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OR

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OF

CHEMISTRY, MATHEMATICS, ASTRONOMY, NATURAL HISTORY, AND GENERAL SCIENCE.

BY

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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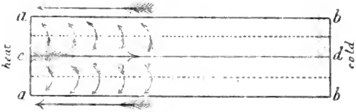

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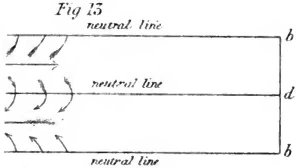

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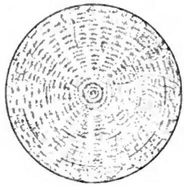

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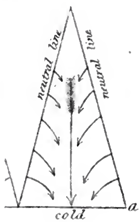

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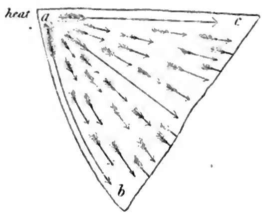

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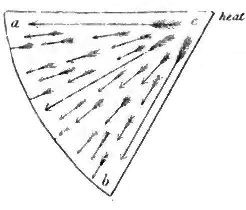

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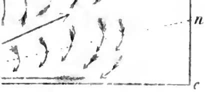

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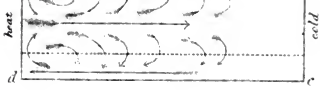

Homogeneous Bodies.

S. Porter Sculp.

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JULY 1831.

I. *On the Thermo-Magnetism of Homogeneous Bodies; with illustrative Experiments.* *By Mr.* WM. STURGEON, *Lecturer on Experimental Philosophy at the Hon. East India Company's Military Academy, Adiscombe*.*

[With a Plate.]

1. EIGHT or nine years have now elapsed since Dr. Seebeck, a Prussian philosopher, unfolded a most important secret of nature, by the discovery of magnetic powers in various metallic combinations, by merely submitting their points of union to different degrees of temperature;—a discovery of equal, if not superior interest, to that of electro-magnetism by Œrsted the illustrious Dane. Each of those discoveries marks a distinct and important epoch in the history of experimental science, and each philosopher now enjoys that degree of fame to which he is so justly entitled.

2. Philosophers in every civilized country have repeated the experiments of those celebrated men,—admired the beauty and interest of the phænomena they present, and vied with each other in adding new facts to those already known. Heat, magnetism, and electricity are now blended in our experiments, and new sciences have been reared upon the phænomena they have jointly presented to our notice.

3. The discovery of Œrsted, and the train of curious and interesting phænomena to which it has directed our view, rest, principally, upon the action of metallic combinations, and a mode of excitation, which to philosophers have long been known;—whilst the discovery of Seebeck, on the contrary, not only depends upon new arrangements, but also upon the novel

* Communicated by the Author.

mode by which he calls forth the electric powers, and interrogates the magnetic phænomena they display.

4. In this mode of research, however, as well as in the former, combinations of two, or more distinct metallic bodies were still employed; and it appears that, by whatever ingenious contrivances other philosophers have pursued the inquiry, combinations of two, or more bodies have generally been considered necessary for the display of thermo-magnetic phænomena. I am not aware, however, that any experiments are yet before the public illustrative of thermo-magnetic action in one solitary piece of metal, or other homogeneous body.

5. In my "Recent Experimental Researches in Galvanism, Electro-Magnetism, &c." a small work lately published, I have stated that simple metallic bodies not only display different electric powers as regards each other, but also that the various *parts* of *each separate metal* are *relatively* in different electric states at one and the same time, although in close connection with each other by the best known conducting material; that is, by the metal itself. And I have shown by several experiments on homogeneous metals, that those magnetic powers, which are regarded as inseparable from the electric, can readily be brought into play either by the galvanic or by the thermo process,—a circumstance, which to me appeared highly confirmatory of the hypothesis.

6. Since writing that work, I have been induced to prosecute those inquiries to a still greater extent; and the experiments and observations which are now about to be described, appear to me to permit no further question as to the existence of thermo-magnetic powers in most, if not in all, of the homogeneous metals; individually, and independently of any connection with each other. And the phænomena they display, are, in many cases, as decidedly obedient to certain unerring laws, as in any thermo-magnetic arrangement whatever.

7. My first experiments for the detection of magnetism by heat in single pieces of metal were not very successful; although the pieces were alloys, and consequently not homogeneous metals. I found however, after some trials, that by hardening one end of a piece of steel, and keeping the other end quite soft, the thermo-magnetic energies were always called into play when any part near to the centre of the bar was heated, and the extremities left at the ordinary temperature of the atmosphere.

8. Brass also, by the same treatment, displays thermo-magnetic properties, which are easily detected by the galvanometer. It is a remarkable fact, however, that the electric current in these metals proceeds in opposite directions as regards the

the hard and soft ends. In cast steel the current in the bar is from the *hard* to the *soft* part; but in brass it flows in the contrary direction. Fig. 1. (Pl. I.) is the shape of the steel or brass bar, which I find very convenient in these experiments: the extremities, which are a little bent, dip into the cups of the galvanometer. There is no occasion, however, to employ a galvanometer with brass; the bar, when heated at the bend, may have its extremities brought into close contact, and one side held over and parallel to the compass-needle; and the nature of the deflection will indicate the direction of the electric current. When the bar is of steel, the direction of the electric current will be indicated by the arrow: when brass is employed, the current flows in the opposite direction.

9. I have magnetized the steel in all the various ways that I could think of; but I have not found that its being a magnet has any perceptible influence, either on the *direction* or the *power* of the electric current excited within itself by heat.

First Class of Experiments with single pieces of Bismuth.

10. Bismuth is one of the metals wherein the magnetic powers are finely developed by heat: the energies are promptly displayed, even by small specimens cast into certain forms; and, as their character can be examined without the aid of a multiplying galvanometer, the phænomena are very easily exhibited. Experiments on bismuth are therefore well calculated to impress immediate conviction on the mind, as to the distinguished and interesting character of the thermo-magnetism of homogeneous bodies.

11. The first piece of bismuth which I employed in these researches was cast into the shape of a rectangular frame, not very unlike the rim of an old-fashioned knee-buckle. Each side of this frame was a rectangular prism, the faces of which were each ·3 of an inch broad. The sides, however, were not very smooth, but no file was employed to level the inequalities. The length of the frame outside measured 3·2 inches, the breadth 1·2 inch. The experiments were made by heating, at different times, various parts of the frame in the apex of the flame of a spirit-lamp: and, when any selected point was thus heated for a few moments, one of the longest sides was immediately held over, and parallel to a delicately suspended compass-needle; on which, the magnetism of the earth was pretty accurately neutralized by means of a distant bar magnet: and the deflections of the needle were taken as an indication of the direction in which the electric currents flowed round the metallic frame.

12. The needle which I have employed is four inches long, furnished with an agate cap, and suspended on a fine steel

point; it is also inclosed in a box with a glass cover. Care was taken to neutralize the metallic frame between every two experiments, by plunging it into cold water.

13. Fig. 2. will represent this frame of bismuth. It was heated successively at the points *a*, *b*, *c*, *d*, close to the angles; but those angles were kept out of the flame of the spirit-lamp. These points were selected for the points of heat, from a notion which I had previously entertained, that heat might possibly be obstructed by turning sharp angles, and thereby influence the direction of the electric currents to which it gave birth.

When the point of heat was at	*a*,	the current flowed from	*b* to *a*
	b,	....	*b* to *a*
	c,	....	*b* to *a*
	d,	....	*a* to *b*.

To prevent confusion, the side *a b*, only, is chosen to show the directions of the electric currents. It is to be understood, however, that those currents were continuous round the rectangular frame.

14. When the experiments were repeated, the direction of the current changed when the point of heat was at *b*; at all the other points the currents proceeded as at first. By varying the situation of the point of heat several times near to *b*, it was found that, when applied anywhere within half an inch of the angle, the current was from *b* to *a* as at first; but when the point of heat was one inch distant from the angle *b*, the current invariably proceeded from *a* to *b*. From these results it was evident, that between half an inch and one inch from the angle, there was a point which, if heated, no electric current would be excited. By various trials this *neutral point* was found to be situated at a little more than half an inch from the angle *b*. So that in general, if all the first half-inch were heated, the current would proceed from *b* to *a*; but if the more distant half-inch from the angle was heated, the current flowed in the opposite direction. Again, as this last current was also in the opposite direction to that excited by heating the point *a*, there would evidently be another *neutral point* still nearer to *a*. This point was determined at nearly half way between the angles *a* and *b*, but a little nearer to the former than to the latter.

15. In this way the situation of the point of heat was varied in the side *c d*. One *neutral point* only on this side was detected, which was nearly half-way between the angles *c* and *d*. If any point in, or the whole of the half nearest to, *c* were heated, the current proceeded from *d* to *c*; but if heat was applied to the other half nearest to *d*, or to any particular point in that half, the current flowed from *c* to *d*.

16. By

16. By consulting the direction of the arrows in fig. 2. the reader will easily ascertain the direction of the electric currents when any particular part of the apparatus is heated; for by heating any point within the range of any individual arrow, that arrow will point out the direction of the electric current. Or, if the whole length of that arrow be heated at the same time, the current still flows in the same direction, and is continued in every part of the frame. The same explanation will also apply to all the other figures, unless otherwise expressed.

Second Class of Experiments.

17. These experiments were also made with a rectangular frame of bismuth exactly of the same length as the former, and about 1·75 inch broad. More care was taken in the casting, and the frame was a better figure.

18. The direction of the arrows in fig. 3. will point out the course of the electric current when any point opposite to them is heated. It will be observed by consulting that figure, that every angle is a *neutral point;* and that the long sides have each one *neutral point,* and when this is heated, no current is excited.

19. There is a circumstance connected with these experiments which is well worthy of remark. On the side *a b* of the rectangle, and near to *b*, there was a protuberance on the inner face: when the point of heat was between this protuberance and the angle *b*, no current was excited; but on the other side of the protuberance being the point of heat, a powerful electric current was put into motion; so that, strictly speaking, the arrow ought not to reach to the angle *b*. The protuberance was afterwards filed down, and that part levelled with the rest of the side: no difference was produced in the thermo-magnetic character of that part of the frame by the change thus made. Hence it would appear, that, the internal structure of the metal alone, operates in giving direction to electric currents excited by heat. This opinion will appear much better supported by experiments and observations, which will be spoken of in the sequel.

20. I must notice in this place, that the thermo-magnetic energies in this rectangle vary considerably by heating different points. When the point of heat is in the side *a b*, the current which sets in from *b* to *a* is much more powerful than the opposite current from *a* to *b*; so that by heating any part in the half nearest to *a*, a stronger current is excited than by heating a point similarly situated with regard to *b*.

21. When either of those halves is uniformly heated, the needle

needle is more deflected than if the heat were confined to a single point. When the whole side *a b* is uniformly heated, in consequence of the superior energies attributable to the half nearest to *a*, the current is directed from *b* to *a*; but the energies of this resulting force are necessarily very feeble.

Third Class of Experiments.

22. Three rectangular frames of bismuth, which I shall distinguish by the letters A, B, C, were cast in the same mould, and from the same mass of metal. The longest side of each frame measured 4·5 inches, and the shorter sides 2 inches. The weight of the whole was 21 ounces; so that the average weight of each was 7 ounces. They were cast under different circumstances, as regards the temperature of the mould, agitation, position, &c.

23. The rectangle A was cast whilst the mould was quite cold; its plane horizontal, and its longest sides parallel to the magnetic meridian. The metal was all poured into the mould (which was simply a groove in one of the flat sides of a Bath brick) at one particular point, which was in one of the longest sides of the rectangle, and about 1·5 inch from one of the angles. The metal consequently flowed from this point to every other part of the mould. Two powerful bar magnets were applied to the two longest sides whilst the bismuth remained in a fluid state; they were drawn from the centre of each long side of the rectangle in precisely the same manner as is practised in magnetizing a needle by the double touch. The process was continued till some time after the metal was set. The mould was kept perfectly at rest all the time.

24. The rectangle B was cast whilst the mould was quite warm from the heat communicated to it by the last casting. The magnetizing process was carried on as before, but the metal was agitated as much as possible all the time. The position of the mould was the same as whilst casting the rectangle A. The magnets in both cases were so applied, that if the magnetism of the earth had any influence in arranging the particles of the fluid metal*, those artificial magnets would have tended to promote that arrangement.

25. The rectangle C was cast whilst the mould was quite hot, and with its longest sides at right angles to the magnetic meridian. No magnet was applied to the bismuth, but continued agitation was kept up till long after the metal was set.

* From some previous observations, I had formed an idea that this might possibly be the case.

26. *Experi-*

26. *Experiments with the rectangle* A.—The arrows in fig. 4. will, in general, indicate the direction of the electric currents in this piece of bismuth. There are, however, circumstances connected with the experiments which require some further explanation.—I shall endeavour to point out these particulars with some degree of minuteness, commencing with those which were observed when the point of heat was on the side *a b*; and proceed successively with the other sides according to the regular order of the letters.

	Side *a b*.	Current.	
Point of heat	close to *a*	none	from *a* to *b*.
	one inch from *a* ...	very feeble ...	
	centre of *a b*.........	more powerful	
	one inch from *b* ...	very powerful	
	close to *b*	very powerful	from *b* to *a*.

Neutral point ·5 inch from *b*.

	Side *b c*.		
Point of heat	close to *b*	powerful	from *b* to *c*.
	centre of *b c*	powerful	
	close to *c*	powerful	from *c* to *b*.

Neutral point ·25 from *c*.

	Side *c d*.		
Point of heat	close to *c*	powerful	from *d* to *c*.
	one inch from *c* ...	feeble	
	centre of *c d*	powerful	from *c* to *d*.
	one inch from *d* ...	more powerful	
	close to *d*	powerful	from *d* to *c*.

Neutral points ·75 inch from *d*, and 1·5 inch from *c*. The latter was the gate or point at which the metal entered the mould.

Side *d a*.—To whatever part of this side of the rectangle heat was applied, the electric current flowed from *a* to *d*; but the energies were more powerful when the point of heat was close to *d*, than when in any other part of the side *d a*.

27. It will be observed that in this rectangle there are only two of the angles, *a* and *b*, which are neutral points. The other angles, *c* and *d*, when heated produce very powerful electric currents; indeed, much more so than when the point of heat is in any other part of the metallic frame. When those two angles are heated at the same time, their energies being excited in the same direction, conspire to produce effect on the needle, which becomes much more deflected than when one angle only is heated. Precisely the same thing happens when any other two or more points are simultaneously heated, the energies

energies of which are directed in one and the same way round the rectangle.

28. In the side *c d* there are two neutral points; one of which is exactly at the gate, or that point at which the metal was poured into the mould. I mention this circumstance more particularly, because I have observed that, when the point of heat is situated on different sides of the gate in any of these frames of bismuth, there are generally opposite currents elicited; or, in other words, the gate is generally a neutral point. The letter *o* in figs. 4. 5. and 6. denotes the gate in each.

29. *Experiments with the rectangle* B.—By consulting fig. 5, it will be observed that the greater part of the side *a b* is neutral. No deflection of the needle could be produced when the point exceeded one inch from either of the angles *a* or *b*.

30. The side *c d* has three neutral points, one of which is at the point *o*, where the metal entered the mould. The two short sides, *b c* and *d a*, have each one neutral point. In *a d* it is at equal distances from the angles *a* and *d*. In *b c* the neutral point is nearer to *c* than to *b*.

31. The opposite angles *a* and *c* are decidedly neutral points; but when the angles *d* or *c* were heated, very powerful currents were excited. The arrows on each side of the angle *b* are both directed the same way, and consequently would represent that angle of one uniform character; but it was found by repeated trials, that there are two neutral points very close to the angle *b*, but on different sides; and so close to each other, that it required a very fine-pointed flame to heat one of those points without heating the other also.

32. *Experiments with the rectangle* C.—There are three neutral points in each of the long sides of this rectangle; one of which is at *o*, the point where the metal entered the mould. In each of the short sides there is one neutral point, situated nearly at their centres, as is shown by fig. 6.

33. The angles *c* and *d* are perfectly inactive when uniformly heated; but the angles *a* and *b*, although represented by the arrows as causing conspiring currents on both sides of each angle, have, in fact, each of them two neutral points very close to each other; so that the needle will be deflected variously by heating different adjoining points about either of those angles.

34. When those parts of the rectangle which are opposite to the arrows *i i* are simultaneously heated, the energies of the conspiring currents become very powerful indeed; and the needle may be driven round on its pivot through a whole circle by following it up with the side of the rectangle. The electric currents excited in this rectangle are more powerful than

than in either of the two former; particularly when heated opposite to the arrows *i i*. The thermo-magnetic powers of A and B, however, when those rectangles were heated at two or more conspiring active points, would frequently deflect the needle over an arc of 30° or 40° by the first impulse.

35. The rectangle A was so exceedingly sensible by the slightest inequalities of temperature in its various parts, that the heat imparted by the finger and thumb by which it was held, would excite an electric current of sufficient energy to deflect the needle 4° or 5°. Indeed, the temperature of the metal was very seldom so far equalized as to render its electric powers completely inert. The natural changes in the temperature of the atmosphere seem to be almost sufficient to perpetuate electric currents, without any artificial change whatever.

Fourth Class of Experiments.

36. As it had appeared from the preceding experiments, that considerable thermo-magnetic action was elicited by bismuth when cast into the shape of rectangular frames, I was desirous to ascertain if those powers were communicated to it by employing it in that particular shape; or if it would still display thermo-magnetic phænomena when cast into other forms. To set this question at rest, I cast several circular rings, or frames of bismuth. The exterior diameter of each ring was 4 inches, and the interior diameter 3·5, leaving the metal ·25 inch thick. They were cast with the plane of the mould horizontal, and open at the upper surface. The height of each ring when in that position was about ·4 inch.

37. By applying the flame of a spirit-lamp to various parts of each ring, and immediately presenting the metal to the compass-needle in precisely the same way as had been done with the rectangles (11.), several active points in each ring were soon discovered, the energies of which were continuous throughout every part of the circular frame, putting the whole circle into a state of thermo-magnetic activity. Several inactive or *neutral points* were also found in these rings, by heating which, no perceptible influence was exercised on the needle by bringing the metal close to it. These results left no question remaining as to the magnetic power being innate and natural to the metal; and not communicated to it by its assuming any particular form. It must certainly be acknowledged, however, that the rings have never displayed the thermo-magnetic energies in so exalted a degree as I have observed in the rectangles; but this difference of energy may possibly be attributable to the difference in the extent of surface which

can be exposed to the needle by having the bismuth in those varieties of shapes; for when the metal is in the shape of a circular ring, it is impossible to bring any more than a very small portion of its surface at any one time sufficiently near to the needle to examine the character of the magnetism which it displays. With rectangles, or other straight-sided frames, it is quite otherwise; for with those, the metal may not only be brought parallel and close to the needle for a considerable extent, but, if the frame be sufficiently long, it can be made to operate on both poles of the needle at the same time, —an advantage of considerable importance in experiments of this delicate character*.

38. Fig. 7. will illustrate the thermo-magnetic character of one of these rings, and will be sufficient, also, to give a tolerably good idea of the thermo-magnetism displayed by similar rings of bismuth generally: although some trifling difference as to the energy, number, and situation of the active points will frequently be found amongst them; for, in the present state of the inquiry, it is next to impossible to procure two exactly alike; nor is it easy to predict an active or an inactive point in a ring of bismuth which is symmetrical in all its parts.

39. It will be observed, by contemplating fig. 7, that the gate *o* is a neutral point. This I have also shown (28) is the case in rectangles; and, indeed, I believe that the gate will always be found in that state, into whatever form of open frames bismuth may be cast.

40. For the convenience of bringing a greater part of the edges

* Should it be asked why the multiplying galvanometer was not resorted to in these delicate experiments, the answer would be, that that instrument, however valuable it may be for some purposes, is quite inapplicable in these inquiries; where every metal, excepting that under examination, should studiously be avoided, and on no account be permitted to enter the thermo-magnetic circuit. Errors frequently occur by employing the multiplier whilst examining the thermo-magnetic character of very small specimens of metals. Besides, the circuit in that instrument is frequently much too long to be penetrated by the feeble energies which are sometimes displayed in homogeneous bodies, but which are easily detected in short circuits, through which they will pass with very great freedom. Doubts, also, regarding the correctness of the results, would necessarily have presented themselves, had any other metal been permitted to enter the circuit; and with very great propriety indeed, might every experiment have been questioned, had the galvanometer, with its copper multiplying wire, mercurial cups, &c. been employed in an inquiry which professes for its object the contemplation of the thermo-magnetism of homogeneous bodies alone. Moreover, these researches, as will presently be shown, have led to discoveries which could never have been made by the employment of the multiplying galvanometer; and the character of several of the experiments is such, as entirely to preclude the use of that instrument in their exhibition.

edges of curvilinear frames as close as possible to the magnetic needle, I cast several into an elliptical form; the diameters of each of which were nearly as to 3 to 1. Every one of these frames became magnetic by heating at various points, in the manner already described; and the thermo-magnetic energies were as promptly displayed in every part of the curve by heating an individual point, as in any thermo-magnetic circuit whatever.

41. I must not omit to mention a very extraordinary circumstance which occurred whilst varying the experiments with one of those elliptical frames; because the same cause frequently produces very singular changes in the thermo-magnetic character of curvilinear and other frames of bismuth.

42. Having proceeded in the usual way (11), I succeeded in detecting several active points in the ellipse. The directions of the various currents which were excited by heating those points are indicated by the arrows in fig. 8; the arrow always pointing in the direction of the current when any point was heated within its length. This done, I made a deep curved notch in the inner side of the rim at the point *d*, fig. 9, by means of the convex side of a half-round file. This small alteration in the edge of the frame caused such a wonderful difference in the activity of its thermo-magnetism as to surpass anything I had hitherto observed. The magnetic energies not only became very much exalted, but in several parts of the frame had completely changed their direction. When heated at the points *o*, or *f*, the deflection of the needle was at least three times greater than before the notch was made. When those two points were heated at the same time, the conspiring magnetic energies thus excited, deflected the needle 50° at the first impulse; and by changing the direction of the frame to promote the deflections, the needle was soon made to sweep an arc of more than 200°; and by following the oscillating needle with the edge of the frame, the former was driven, by the thermo-magnetic forces of the latter, several times quite round the whole circle*.

Remarks

* My motive for making this deep notch in the side of the frame, was that of checking the progress of the heat in one direction more than another, when applied to either side of it,—an object which I had in view from the commencement of these experiments, and which I had in vain attempted to accomplish by means of angular points (13); and from frequent disappointments by the apparently fortuitous results in the preceding, and many other experiments which I have not mentioned, I had lost all hope of arriving at anything like interposition, in any other way than that of modifying the crystallization of the metal. The results of this experiment, however, were very singular; and I have since found, that

Remarks on the preceding Experiments.

43. It would be needless (indeed almost endless) to describe all the peculiarities which have attended the numerous repetitions of the preceding experiments; and the variety of circumstances under which I have cast rectangular, triangular, and curvilinear frames of bismuth; as the phænomena already described, with a few summary remarks, will pourtray their general character.

I am not certain that any peculiar property is communicated to bismuth by the magnetic process (23), or by the position of the metal as regards the cardinal points, whilst casting. I have frequently cast rectangles under these circumstances, but have not found more regularity in the display of their thermo-magnetism than in those which were cast under circumstances quite different.

In general, small, and well-formed rectangles (17) (fig. 3.) operate with much greater regularity, as regards the angles, than those which are of larger dimensions (figs. 4, 5, 6). But it frequently happens that the energies of the latter are much superior to those of the former.

The currents, more frequently than otherwise, proceed in opposite directions, when excited on different sides of an angle. And in those cases where it happens otherwise, the neutral point is situated almost close to the angle.

The gate is invariably a *neutral point* when the mould is not very hot at the time of casting, and more frequently than otherwise in castings generally.

The thermo-magnetic character of frames of bismuth, whether *angular* or *curvilinear*, may be considerably modified by removing, or partially removing, crystalline groups, or by altering their forms. (41, 42 and note.)

Every part of a continuous ring or rectangular frame of

similar results frequently happen from the same cause: this however is not always the case; it takes place most frequently in those frames which, whilst whole, are not very active; and a neutral point ought to be selected for the situation of the notch. I have frequently reduced particular parts of circular rims of bismuth with a hot iron, and sometimes with the flame of a spirit-lamp, instead of a file, and with similar results.

There is another method of exhibiting a very remarkable and nearly allied property of thermo-magnetism, much more decidedly than by that of filing,—by experiments which do not properly belong to this class, although they were suggested by the results of the experiment last described; besides, there are impediments of which I must warn the reader, before I can possibly describe the method of arriving at anything like success. The circumstances, however, which I have called impediments, emanate from the display of a very interesting class of phænomena, which I shall presently describe.

bismuth

bismuth becomes magnetic by heating the smallest possible point.

The phænomena generally, are sufficiently striking to be exhibited in a lecture-room, and to be observed by the most distant auditor, provided the needle be neutralized. Two, or more rectangular frames may be selected, and properly adjusted side by side, so that their combined energies may be made to operate simultaneously on a very large needle. In this way the experiments may be made on a very extensive scale.

Fifth Class of Experiments.

44. The experiments which I am now about to introduce present phænomena, perhaps somewhat less complicated, though by no means less curious, than those I have already described.

I had observed, whilst experimenting with a rectangular frame of bismuth of large dimensions, that the needle would sometimes be deflected in one direction and sometimes in the other, even when the point of heat was not varied. Struck by this unusual phænomenon, I proceeded to examine it with some degree of minuteness, and with an intention of ascertaining its cause.

45. Before noticing this apparent anomaly, I had constantly held the plane of each rectangle in the plane of the magnetic meridian, and with its lower edge as close as possible to the needle,—a position, which I considered as the most likely to obtain true results, because, when so placed, the upper edge of the frame, in consequence of its great distance, could not affect the deflections of the needle produced by the thermo-magnetic forces in the lower side. This position of the frame would certainly have been better than any other for ascertaining the direction of single electric currents, or those thermo-magnetic forces which are circumfused in the apparatus of Seebeck, and other similar combinations; although a slight degree of inclination either to the east or west, with such compound apparatus, would not very materially have affected the results. I soon became convinced, however, that the anomaly which I had noticed proceeded entirely from that cause; for if the plane of the rectangle inclined to the east, the needle would be deflected in a contrary direction to that which it assumed, by inclining the plane of the rectangle to the west. By several trials it was found, that when the plane of the frame became nearly horizontal eastward (still keeping the end north), a greater deflection was obtained than by holding it at any other angle on that side of the meridian. And by placing it in a similar position to the west of the meridian,

ridian, the greatest deflection was again produced in the opposite direction to the former.

46. Considering this as one step at least towards the discovery of the cause of this novel phænomenon, I proceeded to examine the opposite side of the frame, which I supposed might possibly present similar effects. For this purpose I heated one of the points in that side which by previous trials was found to be very active, and then placed it directly over the magnetic needle, keeping the plane of the frame in the magnetic meridian, and the same end north as had been in that position whilst experimenting with the other side. The needle was deflected in precisely the same direction as I had always found it to be by a similar position of the frame, and with the same point of heat. I afterwards inclined the plane of the frame sometimes eastward, at others to the west; but in no instance could I obtain the least indication of results similar to those which I had noticed whilst the opposite side of the frame was nearest to the needle. Thinking that the different active points might possibly operate under different laws, I next heated the point which had before presented the extraordinary phænomenon, still keeping the other side nearest to the needle; but nothing remarkable was noticed by this variation of the experiment. The needle continued to be deflected in one uniform direction, whatever inclination was given to the plane of the rectangle. I tried the other sides of the rectangle in precisely the same way, but obtained no unusual results. It now appeared evident that one side of the frame was endued with peculiar properties which the other sides could not be made to exhibit, which soon proved to be the fact.

47. When this side was heated at one particular point, two distinct electric currents were called forth; one of which may be distinguished by the name of *general* current, because it pervaded every part of the metallic frame; the other current was perfectly *local*, and could be traced only to a short distance from the point of heat. It never reached further than the angle, and returned into itself on the opposite face of the solid prism which formed that side of the frame.

48. From this singular result, it appeared likely that some inaccuracy might possibly have occurred in the conclusions I had already drawn from the former experiments. Fortunately the rectangles were not broken, and I had an opportunity of examining them again: but it seems that they were of too small dimensions to produce *local* currents, as I found them to operate exactly as at first. On trying one of the circular rings, however, another curious fact was discovered, which

which at that time was not a little surprising. A reference to fig. 10. will assist the illustration of this singular property discovered in the ring of bismuth.

49. When the outside of the ring at *b* was held for a few moments in the point of a well-defined flame of a spirit-lamp, the electric current in every part of the ring was in the direction of the exterior arrow; but when the inner side of the rim opposite to *b* was similarly heated, the current proceeded in the contrary direction, as indicated by the interior arrow. And so directly opposite and close to each other were these two active points, that a transverse section of the metal at the point *b*, would have embraced them both. I have since that time observed active points similarly situated in other pieces of bismuth.

50. *Experiments with a solid Prism of Bismuth.*—That side of the rectangle (47) which had exhibited *local* electric currents was now cut from the frame, for the purpose of ascertaining whether phænomena similar to those described (47) would be exhibited by this prism when examined as a distinct individual mass. Fig. 11. will represent this prism or bar of bismuth: when heated at *b*, the former active point, the bar became highly magnetic, displaying those energies in a very exalted degree. The electric current was traced from the point of heat towards the end *c*, along that side of the prism which in the figure is hid from view. It proceeded across the end *c*, as shown by the curved arrow; and returned to the heated point along the front face of the prism: so that when heated, if the front face *a*, *b*, *c* were placed over and parallel to the needle, and the end *c* north, the north end of the needle would be deflected towards the east; and by simply turning the prism the other side up, the needle would be deflected towards the west. By thus turning the bar, at suitable times to promote the deflections, the needle could be made to sweep an arc of 90°.

51. The other two faces of the prism were nearly neutral, each of them partaking of the opposing currents in the active sides.

52. The bar was afterwards heated at various parts of its surface, and the direction and energy of the currents ascertained by the deflections of the compass-needle. It was ultimately found that when the whole of the end *c* was uniformly heated, the most powerful current was excited; its course was still in the same faces of the prism, but its direction was reversed.

53. When the end *a* was uniformly heated, no thermo-magnetism was elicited, until, as the heat advanced in the bar, the temperature became disturbed at the point *b*. This accomplished, those powers were again roused from their quiescence,

escence, and displayed phænomena precisely of the same character as when the bar was heated at the point *b* only.

54. The neutral end *a* was now cut off at the point *b*; and the remaining bar displayed thermo-magnetism to whatever end heat was applied. The currents still passed over the same faces, and, as regards the point of heat, uniformly set out over the same face and returned by the other. Hence the apparent contrariety in its direction when the bar was heated at different ends. These results led to further experiments with bars, and other forms of solid homogeneous metallic masses; some of which have developed very curious phænomena, which appear to observe an uniformity in the laws of their exhibition.

55. *Experiments with a cylindrical Bar of Antimony.*—This bar was 8 inches long and ·75 inch diameter. It was very far from having an uniform surface, being much more cavernous on one side than the other, from air-bubbles whilst casting. It was heated at various points of its surface by a very fine-pointed flame of a spirit-lamp; and its magnetism thus excited was traced by its action on the compass-needle. It was ultimately discovered, that whatever point near to the extremities of the cylinder was selected for the point of heat, the electric current invariably flowed over the same parts of its surface; and when either of the ends was uniformly heated, whilst the other was kept at the temperature of the atmosphere, the bar became highly magnetic; exhibiting phænomena similar to those already spoken of as appertaining to the prism of bismuth (54). The electric current constantly passed over the dense and opposite cavernous sides, whilst the intermediate longitudinal lines were nearly neutral.

56. Fig. 12 will give a good idea of the direction of the electric currents excited in this bar of antimony when one of its ends was uniformly heated. The cylinder is supposed to be divided into halves in the plane of its axis, and terminating on each side by the dense and cavernous parts of its surface. The two halves are placed edge to edge, with their convex sides upwards. The dotted line in each half will represent the neutral line, or that longitudinal line on each side of the bar, which when placed parallel over the needle, no deflection was produced. The active lines are *c d*, and *a b*, *a b*: the two latter correspond to each other when the halves of the cylinder are replaced or brought together. It is to be observed, however, that the thermo-magnetic energies of the cylinder were not confined to two longitudinal lines; for every part of the surface near to the heated end was more or less magnetic; but in consequence of the recurved manner in which

which the electric currents flowed over the surface, there were necessarily two longitudinal lines more active than any other part. These lines passed through the dense and opposite cavernous sides, and may be termed the lines of *greatest energy*. The *neutral* lines were also a consequence of the recurved flow of the electric currents by intersecting them at right angles; and as those intersections were in a series of points nearly parallel to the axis, those neutral lines were nearly parallel to the axis also.

57. When the other end of the cylindrical bar was heated, the lines of *greatest energy* were still on the same parts of the surface, but the electric currents flowed in an opposite direction to the former; so that to whatever end of the bar heat was applied, the current uniformly proceeded from the heated end along the dense side *c d*, and returned over the opposite or cavernous part of the surface *a b*, *a b*.

58. The thermo-magnetic energies never reached to the cold end of the bar, but returned to the point of heat in directions indicated by the arrows, and at no great distance from the heated end. The same laws hold good in all cylindrical bars of antimony of small dimensions, which are not of an uniform density on every side of the axis. I have broken several into fragments for the purpose of examining their internal structure, and have always observed, that when they have displayed phænomena similar to those last described, their density is not uniform; and the side of the cylinder, which in a transverse fracture will exhibit the most compact texture, may generally be predicted by an observance of the thermo-magnetic phænomena which it will display whilst whole.

59. Cylindrical bars of antimony of an uniform density on every side of the axis, and more than two inches diameter, display thermo-magnetic phænomena with very great precision, and a rigid observance of certain laws.

60. When a bar of this description, and 6 or 8 inches long, has been cast in a vertical mould of sand, let its ends be struck off with a sharp-edged hammer, making the sections transverse and not ragged. Apply the flame of a fierce spirit-lamp for a few moments to the convex surface close to one end of the cylinder, and immediately place the heated side downwards, over and parallel to a delicately suspended compass-needle. If the heated end of the bar be placed north, the north end of the needle will be deflected *eastward*, showing that the electric current by which it is deflected is flowing along the lower side of the bar, from the point of heat towards the cold end.

61. Let

61. Let the same point be again heated in a similar manner, and again place the cylinder over the needle with its heated end north, but with the point of heat upwards. The north end of the needle in this case will be deflected towards the west, or in the opposite direction to that which it assumed in the former experiment. This deflection of the needle indicates the electric current to be flowing in the cold side of the cylinder from south to north, or in the opposite direction to that in which it flows in the heated side.

62. Heat the same point again: but instead of placing the heated or opposite side of the cylinder over the needle, as in the former cases, place one of those parts of the surface over it, which is about 90° from the point of heat, still keeping the cylinder parallel to the needle. In this position the latter is scarcely affected; and by a few trials a line will be found on the surface of the cylinder, and nearly parallel to its axis, which has no action whatever on the needle. This is one of the *neutral* lines; and by a few trials on the other side of the point of heat, another *neutral* line will be discovered. These lines are generally at about 90° from a line drawn from the point of heat to the other end of the bar, and parallel to the axis.

63. This latter line is one of the lines of greatest energy; the corresponding line of greatest energy being parallel, but on the opposite side of the cylinder (56).

64. Similar phænomena will be displayed by making any other part of the convex surface near to the ends of the cylinder the point of heat. The current uniformly flows over the surface on the heated side from the point of heat, expands into two distinct tides which sweep the surface of the metal, and reuniting on the opposite side recurves into itself at the heated point of the cylinder.

65. The general distribution of the electric force on the surface of the cylinder by heating it as directed (60, 61, 62), will be pretty accurately indicated by the arrows in fig. 13. The cylinder is supposed to be divided into halves, and its convex sides upwards, as in fig. 12 (56). The straight arrows indicate the lines of greatest energy; and the edges *a b*, *a b*, which coincide when the halves are replaced, are in one of the *neutral* lines: the other *neutral* line is *c d* in the centre of the figure.

66. The thermo-magnetic energies can hardly ever be traced more than four inches from the point of heat: they are, however, excited to a certain extent by the slightest disturbance of temperature near to either of the ends of the cylinder.

67. When

67. When any point is suddenly made pretty hot, without elevating the temperature of the opposite side, which can easily be done when a cylinder is employed of more than two inches diameter, the electric force is very considerable, and will deflect the needle to an angle of 20° or 30°; and by dexterously turning it the other side up before the returning needle arrives at the magnetic meridian, another impulse is given, and the angle increased on the other side. Two or three turns of the cylinder in this way will cause the needle to sweep a considerable arc; but the arc over which the needle passes will be very much increased if the needle be followed up by the active sides of the cylinder, still keeping the one parallel to the other.

Remarks on the preceding Experiment.

68. There is a peculiarity in the phænomena displayed in this experiment which has not been observed in any of the rest:—When a cylinder of antimony is cavernous on one side, I have shown (57) that the electric current invariably flows over the same parts of the surface; but in cylinders of uniform density on every side of the axis, the law of thermo-magnetic action is very different, and the route of the electric current over the surface of the metal entirely depends upon the situation of the point of heat.

69. When a cylindrical bar of antimony is uniformly dense on every side of its axis, it will invariably present a regular crystalline form at every transverse fracture. The general contour of the section is that of a series of exceedingly thin, concentric crystalline laminæ, of which the whole face of the fracture seems to be composed from the centre to the surface of the cylinder. Aided by a magnifier, the eye is enabled to trace apparent radiating veins, which by close inspection are observed to separate the laminæ into distinct parcels or tall narrow bundles, with their edges inclined to each other at various angles, both salient and re-entering: and the apparent veins, which are frequently nothing more than an angle at which two bundles of laminæ unite, give to the fracture a beautiful glittering appearance. Some of those radiant veins, however, are absolutely the flat facets of laminæ, or more frequently the sloping edges of bundles of them, which have a brilliancy far superior to any other part of the fracture. The general position of the laminæ, however, is, that their flat surfaces are presented to the axis of the cylinder; and although there are certainly objections to this position being uniformly determined by the crystalline laminæ, because of several of the piles or bundles being posited at various angles, yet the

 major

major part of those piles are absolutely set in that position, and not a single crystalline film has its plane determined at right angles to the axis of the metal. Hence it is that the edges of the greatest part of the bundles of laminæ are presented to view at every transverse fracture, and may be compared to tall narrow bundles of thin metallic leaves, or slips of paper, placed round a central nucleus, with one of their narrow ends presented to the centre and the other towards the surface of the cylinder; which position, together with others which some of those bundles assume, give to them the appearance of radii, with various degrees of splendour. Fig. 14. will assist in giving an idea of the general disposition of the strata of crystals in a transverse section of an uniformly dense cylinder of antimony.

70. If the sharp edge of a hammer be applied in the direction of the axis, the cylinder may be completely dissected from its surface to its centre; or the crystalline layers may be peeled off one after another with very great accuracy, as far as the dissection is required to be carried on. When a cylinder of antimony is thus disrobed, it presents an exceedingly beautiful appearance: the refulgent facets of its crystals are exposed to view, which stud its surface as if it were decked in a most brilliant coat of mail; whilst the multitude of spangles which those facets display are now seen to be disposed in the crystalline arrangement already described.

71. Assuming, then, that the general crystalline arrangement is that of concentric laminæ, two hypotheses may perhaps present themselves for an explanation of the thermo-magnetic phænomena elicited in an uniform cylindrical bar of antimony, one of which, it appears to me, will ultimately be found to be the true theory.

72. First, then, it may be supposed that the opposite faces of each metallic film are in different states of electricity, or at least that they have different thermo-magnetic qualities. If it could be satisfactorily proved that this were the case, their concentric arrangement would reconcile the phænomena to all those which are displayed by the juxta-position of any pair or series of pairs of dissimilar metallic plates, and each bundle of films would become an electric column. In that case the thermo-magnetic character of the inner surface of each film would be to its outer surface as bismuth is to antimony; for the current in a pair of those metals flows through the point of heat from the former to the latter; and the rest of the circuit answers no other purpose than that of a conductor. When the point of heat is close to the edge of a transverse fracture of a cylinder of antimony, two, or a very few

few more of these plates or crystalline films may possibly be the only parts excited; and the rest of the bar assume the character of a conductor only; in which case the current would flow, at the point of heat, across the films from the internal to the external parts of the cylinder; the direction which experiment discovers it to proceed in: besides, it is possible that the crystalline laminæ may individually have different electric powers.

73. The other hypothesis supposes, that as the crystalline strata are only in juxta-position, and not very firmly united, it is possible that the heat applied at any point on the surface of the cylinder would meet greater obstacles in its progress whilst passing from film to film than any which it would fall in with whilst flowing over the surface of those films, or over the general surface of the metal: and as heat is well known to affect electrical phænomena generally, and as it is the exciting agent in this particular class, it may be supposed that, by its travelling at different rates in those directions, the electric powers of the metal may also be put into motion, and assume certain uniform directions as regards the directions in which the heat flows with the *greatest* and *least* facility*.

74. *Experiments with solid Cones of Antimony.*—When a solid cone of antimony, uniformly dense on every side of its axis, is made the subject of experiment, the surface near to its base displays thermo-magnetic phænomena of precisely the same character as those which have been described in the experiments with a cylinder (60, 61, 62).

75. The cones which I have employed were 4·5 inches high, and the diameter of the base 2·25 inches.

76. When any of these cones were heated at any point of the side near to the base, the current uniformly proceeded from the point of heat over the surface towards the apex, and returned on the opposite part of the surface to the base. This was the direction of the lines of greatest energy, but like the cylinder, the surface of the cone becomes generally thermo-magnetic by this process, and the direction of its forces are easily traced by the compass-needle.

77. Fig. 15. will represent the surface of a cone of antimony in a state of thermo-magnetic action: the cone is supposed to be divided into halves from its apex to its base, and in

* These hypotheses are offered merely as conjectures, without any intention of insisting on either of them, until experiment affords more data in their favour. If I mistake not, however, some of those which I have yet to describe will bear directly on the subject.

the

the plane of the neutral lines. The same explanation will apply to this figure as to the cylinder, fig. 13. (65.)

78. It is not necessary that the point of heat be exactly in the edge of the base to produce the greatest effect, for the direction of the electric force is still the same, and quite as energetic, when the point of heat is at some short distance from the base. Neither is it necessary that any point be made very hot, unless it can be done very suddenly; for the powers excited are decisively exhibited when the selected point of heat is held only for a few moments in the apex of the flame of the spirit-lamp, and the cone immediately applied to the compass-needle, before the heat has time to spread, to any great extent, over the conical surface.

79. When the apex of the cone of antimony is heated, the electric force is exceedingly feeble, and its direction quite uncertain. In general, the thermo-magnetic forces displayed by heating any point nearer to the apex than to the base are comparatively insignificant, and their directions not easily predicted.

80. A cone of antimony which had exhibited the phænomena already described, was cut in two by a saw, at about 1·5 from the apex, and parallel to the base. The small cone operated precisely as the original one, of which it was a part; but the energies were by no means so powerful.

81. The frustum presented phænomena as if it had been a complete cylinder, and the electric currents were as decidedly traced when the point of heat was near to the section as when it was near to the base.

82. When cylindrical bars or cones of bismuth are experimented with in the manner I have described with antimony in those shapes, the thermo-magnetic phænomena are precisely of the same character, and are regulated by the same laws; so that whatever phænomena be displayed by the one metal will also be displayed by the other, provided the cylinders or cones be well cast, and of uniform density on every side of the axis.

83. In bismuth, however, it sometimes happens that in consequence of an irregularity of crystallization, which it is prone to assume, there will be one point, and sometimes two, which when heated will display thermo-magnetic phænomena very different to those I have before spoken of; but these are irregularities which have nothing to do with the general character of the phænomena, and but seldom occur.

84. *Observations.*—Whatever peculiarities there may be in the crystallization of antimony and bismuth when in masses of

of other forms, they exhibit arrangements exceedingly similar to each other when cast into cylinders, which are regularly and uniformly cooled on every side; and there is so little difference in the general aspect of a transverse fracture of the two metals, that were it not for the difference of colour, it would require some practice to distinguish the one from the other. From this circumstance it appears highly probable, that the same cause, whatever it may be, is the fountain of the thermo-magnetism in both metals.

85. It has been intimated to me by some very scientific gentlemen, that impurities in the metal may possibly be the cause of all the thermo-magnetic phænomena which I have attributed to homogeneous bodies; and I must confess that for some time I had entertained a similar opinion: experience and observation, however, by no means sanction the concession. Some other cause than that of impurity in the metal is unquestionably in active operation; and to some other cause we must direct our attention before we can accomplish an explanation of the phænomena in question. A very small portion of tin added to bismuth, not only dispossesses it of its magnificent crystalline ramifications, but also of the superlative display of its natural innate thermo-magnetism: moreover, that small morsel of tin not only paralyses the thermo-magnetism natural to bismuth as a homogeneous metal, but absolutely transfers its thermo-magnetic character as regards other metals, from one extremity of the range to the other; so that if pure bismuth be regarded as the most positive metal, its alloy with tin will be the most negative substance, either simple or compound, with which we are acquainted; and antimony, which has hitherto claimed the negative extremity of the range, is highly positive to this simple alloy.

86. The thermo-magnetism natural to antimony becomes completely stagnated when mixed with tin or lead, and the crystals of the metal become insignificant shapeless specks. Zinc also, which when in larger masses displays its innate thermo-magnetism in a degree superior to any other metal except antimony and bismuth, becomes comparatively inert by a mixture of tin or lead.

And what perhaps may appear a more convincing fact than all the rest is, that antimony and zinc, which separately, as homogeneous bodies, display fine crystalline forms, and also active thermo-magnetism, will, if mixed together as an alloy, become robbed of both those distinguished characters at once, and the resulting metal appears as compact as the finest steel.

87. Whatever may be the notions entertained as regards the

the mass or quantity of metal employed in heterogeneous thermo-magnetic combinations, I find that in the display of the thermo-magnetic phænomena of homogeneous bodies, the quantity employed is an essential consideration; for in several of the metals, although no trace of thermo-magnetism can be detected in small pieces, its powers are promptly developed in masses of considerable dimensions, and the laws of its phænomena may be determined with precision. Zinc, when in large masses, displays thermo-magnetic phænomena in a very exalted degree, but in small pieces hardly any trace of that power is to be found. Copper is a still more striking instance of the superior thermo-magnetic powers of large masses. Those powers could not be detected in a few ounces of the metal; but in a mass weighing 60 or 70 pounds, they would become very conspicuous. But a mass of copper of a hundred weight, however heated, would not deflect a needle half so far as it would be deflected by a single ounce of bismuth or antimony. Yet, insignificant as these powers are in some bodies, I have succeeded in detecting them in every metal of which I had a sufficient quantity at command; and I have no doubt that they may be discovered in all the metals.

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[To be continued.]

II. *On Simple Elimination. By* J. E. Drinkwater, *Esq.**

(1). THE theory of elimination is intimately connected with the general theory of the resolution of algebraical expressions, and has engaged the attention of mathematical writers in a corresponding degree. When the number of equations and of unknown quantities is considerable, the labour necessary for the extrication of any one of them becomes very great, even in the case of simple equations,—a case which is perpetually recurring in physical investigations. The well-known rule given for this purpose by Bézout will be found on trial more laborious than it appears in its concise enunciation, and no simple demonstration has yet been given of this or any analogous general theorem. The investigation by Laplace in the *Mémoires de l'Académie des Sciences*, 1772, p. 2, which is there only incidentally introduced, is both diffuse and difficult. The following method of obtaining the final equations may be easily identified with those of Laplace, Bézout, and Cramer, although the coefficients are obtained in rather a different and, as it is thought, a more convenient form.

* Communicated by the Author.

(2). Let

(2.) Let the equations be

$$A_1 + X_1 x + Y_1 y + Z_1 z + T_1 t + \dots (n) = 0$$
$$A_2 + X_2 x + Y_2 y + Z_2 z + T_2 t + \dots (n) = 0$$
$$\vdots A_n + X_n x + Y_n y + Z_n z + T_n t + \dots (n) = 0$$

the number of equations and of unknown quantities being n, and X_m representing the coefficient of x in the mth equation.

(3.) Write down the series of natural numbers 1 2 3 4 ... n, and underneath it all the permutations of these n numbers, prefixing to each a positive or a negative sign according to the following condition:

Any permutation may be derived from the first by considering a requisite number of figures to move from left to right by a certain number of single steps or descents of a single place. If the whole number of such single steps necessary to derive any permutation from the first be even, that permutation has a positive sign prefixed to it; the others are negative. For instance, 4213 ... n may be derived from 1234 ... n, by first causing the 3 to descend below the 4, requiring one single step; then the 2 below the new place of the 4, another single step; lastly, the 1 below the new place of the 2, requiring two more steps, making in all four. Therefore this permutation requires the positive sign.

(4.) The same permutation may be derived in various ways, and it is necessary therefore to show that this rule is not inconsistent with itself: thus the same permutation 4213 ... n might have been obtained by first marching 1 through three places, then 2 through two; and, lastly, 3 through one, making six in all, an even number as before. Without accumulating instances, it is plain, if q be the smallest number of steps by which any number p reaches the place it is intended finally to occupy in that permutation, that if p should advance in the first instance m places beyond this, it must subsequently return through m places; or, which is the same thing, it must at a later period of the march, allow m of those which it has passed to repass it, so that it will regain its proper place, after the number of steps has been increased from q to $q+2m$, which, by the rule, require the same sign as q. The same reasoning applies to every other figure; and hence the consistency of the rule is evident.

We may, therefore, derive any permutation from any other by altering the sign of the one chosen, according to the change made in its order, and generally it will be found convenient to derive each from the one immediately preceding it.

(5.) We thus get the series $+1234 \dots\dots n$
$-2134 \dots\dots n$
$+2314 \dots\dots n$ &c.

 Take

Take the product $XYZT\ldots\ldots(n)$ of all the letters coefficient of the unknown quantities, and affect it with the successive terms of this series (with their proper signs), considered as a series of indices. We thus form the series

$$\begin{array}{l} +X_1Y_2Z_3T_4 \ldots\ldots (n) \\ -X_2Y_1Z_3T_4 \ldots\ldots (n) \\ +X_2Y_3Z_1T_4 \ldots\ldots (n) \\ \quad\&c. \end{array}$$

which we shall denote by $f\{XYZT \ldots (n)\}$

(6.) It will be observed in all the terms of $f\{XYZT\ldots (n)\}$ that the order of the factors remains unchanged, the indices alone being permuted. If any change is made in the order of the factors under the symbol, it is obvious that the only possible change produced will be a change of sign, which will become positive or negative according to the same rule of signs already explained among the indices. For instance

$$f\{XZYT \ldots (n)\} = -f\{XYZT \ldots (n)\}$$

and so of any other. An odd number of single steps by the requisite number of letters requires the negative sign; an even number, the positive sign. The entire order of the factors may be inverted in $\frac{n.(n-1)}{2}$ single steps, and accordingly as this number is even or odd, $f\{XYZT\ldots(n)\} = \pm f\{(n)\ldots TZYX\}$.

(7.) The function of n factors $f\{(n)\ldots\ldots TZYX\}$ may be expressed in a series of functions of $n-1$ factors, by considering that the permutations of n numbers are obtained by successively adding each at the end of the permutations of the $n-1$ others. Attending also to the change of sign necessary to bring each factor in succession to the end of the product under the symbol, which is necessary in order to keep the products of n factors in the same order in each term so obtained,

$$f\{(n) \ldots TZYX\} = X_n f\{(n-1)\ldots TZY\}$$
$$-Y_n f\{(n-1) \ldots TZX\} + Z_n f\{(n-1) \ldots TYX\} - \&c.$$

or inverting the order of the factors under the symbol in every term,

$$f\{XYZT\ldots(n)\} = \pm X_n f\{YZT \ldots(n-1)\}$$
$$-Y_n f\{XZT \ldots(n-1)\} + Z_n f\{XYT\ldots (n-1)\} - \&c.\}$$

The sign of the first term on the right-hand side of this equation may be made different from that of all the others, by making X under the symbol successively occupy every place from the first to the last, the order of the others remaining unchanged.

We

We thus get,

$$f\{XYZT...(n)\} = \pm\, X_n f\{YZT\ ...\ (n-1)\}$$
$$-Y_n f\{XZT...(n-1)\} - Z_n f\{YXT\ ...\ (n-1)\} - \&c.\}$$

(8.) If any factor in $f\{XYZT...(n)\}$, as X, be divided into two parts, $X = V + W$, the function may be similarly divided, so that

$$f\{(V+W).YZT\ ...\ (n)\} = f\{VYZT\ ...\ (n)\} + f\{WYZT\ ...\ (n)\}$$

placing each part of X in the same relative position (which in this example is the first) which X itself occupied before the division.

(9.) If any quantity which does not vary from one equation to the other, and which therefore is not liable to be affected with an index, is found under the symbol, it may be considered a constant coefficient of every term of the developed function; and written as such on the outside of the symbol: of this nature are the unknown quantities themselves, so that for instance, $f\{XYxZT\ ...\ (n)\} = x f\{XYZT\ ...\ (n)\}$ and so of like quantities.

(10.) Premising these properties of this function, it is easy to show that the final equations derived from those proposed in (2) are $f\{AYZT\ ...\ (n)\} + x f\{XYZT\ ...\ (n)\} = 0$

$$f\{XAZT\ ...\ (n)\} + y f\{XYZT\ ...\ (n)\} = 0$$
&c.

(11.) In the proposed equations let $A + Xx = B$, and suppose the theorem proved for $n-1$ equations, we have,

$$\begin{aligned}
&B_1 + Y_1 y + Z_1 z + T_1 t + \ldots (n-1) = 0\\
&B_2 + Y_2 y + Z_2 z + T_2 t + \ldots (n-1) = 0\\
&\vdots\\
&B_{n-1} + Y_{n-1} y + Z_{n-1} z + T_{n-1} t + \ldots (n-1) = 0\\
&B_n + Y_n y + Z_n z + T_n t + \ldots (n-1) = 0
\end{aligned}$$

From the $n-1$ first of these we get, by the supposition,

$$f\{BZT\ ...\ (n-1)\} + y f\{YZT\ ...\ (n-1)\} = 0$$
$$f\{YBT\ ...\ (n-1)\} + z f\{YZT\ ...\ (n-1)\} = 0$$
&c.

Multiplying the first of these by Y_n, the second by Z_n, &c., we get, by comparison of the result with the last of the proposed equations,

$$B_n f\{YZT...(n-1)\} - Y_n f\{BZT...(n-1)\} - Z_n f\{YBT...(n-1)\} - \&c. = 0$$

We have already seen (7) that this expression is equivalent to $\pm f\{BYZT\ ...\ (n)\}$

$$\therefore f\{BYZT\ ...\ (n)\} = 0, \text{ or restoring the value of } B$$
$$= f\{(A + Xx).YZT\ ...\ (n)\}$$

dividing

dividing the value of B (8), and withdrawing x from under the symbol, (9)

$$f\{AYZT\ldots(n)\} + xf\{XYZT\ldots(n)\} = 0$$

which establishes the theorem to be true for n, if true for $n-1$ equations. The simple equation $A_1 + X_1 x = 0$ evidently conforms to the rule, which is therefore true when $n = 1$; it is therefore generally true.

(12.) The rapidity of this method is best estimated by examples. Take three equations, in which by the rule

$$\frac{f(AYZ)}{f(XYZ)} + x = 0$$

The series of indices is $+123-132+231-213+312-321$; therefore

$$\frac{A_1(Y_2Z_3-Y_3Z_2)+A_2(Y_3Z_1-Y_1Z_3)+A_3(Y_1Z_2-Y_2Z_1)}{X_1(Y_2Z_3-Y_3Z_2)+X_2(Y_3Z_1-Y_1Z_3)+X_3(Y_1Z_2-Y_2Z_1)}+x=0$$

For four equations the series of indices, deriving each from the one preceding it, is $+1234-1243+1342-1324+1423-1432+2143-2134+2314-2341+2431-2413+3124-3142+3241-3214+3412-3421+4132-4123+4213-4231+4321-4312$

and since $x + \dfrac{f(AYZT)}{f(XYZT)} = 0$

$$x + \frac{\begin{array}{l}A_1\{Y_2(Z_3T_4-Z_4T_3)+Y_3(Z_4T_2-Z_2T_4)+Y_4(Z_2T_3-Z_3T_2)\}+\\ A_2\{Y_1(Z_4T_3-Z_3T_4)+Y_3(Z_1T_4-Z_4T_1)+Y_4(Z_3T_1-Z_1T_3)\}+\ \&c.\end{array}}{\begin{array}{l}X_1\{Y_2(Z_3T_4-Z_4T_3)+Y_3(Z_4T_2-Z_2T_4)+Y_4(Z_2T_3-Z_3T_2)\}+\\ X_2\{Y_1(Z_4T_3-Z_3T_4)+Y_3(Z_1T_4-Z_4T_1)+Y_4(Z_3T_1-Z_1T_3)\}+\&c.\end{array}} = 0$$

III. *On the Solar Eclipses and Transit of Mercury over the Sun's Disc, in* 1832. *By Professor* Encke *.

In 1832 there will be two solar eclipses, and a transit of Mercury. The latter phænomenon only will be visible at Berlin. No lunar eclipse will happen.

Solar Eclipse 1832, *February* 1. App. time of Berlin.

Beginning on the earth in general........................ 8^h $6'$
in 176° 52′ east longitude from Ferro.
8 49 south latitude.

* From the Berlin Astronomical Ephemeris for 1832, page 200—205.

Beginning

	App. time of Berlin.
Beginning of the central (annular) eclipse	$9^h\ 13'$
in $160^\circ 23'$ east longitude from Ferro.	
8 8 south latitude.	
Annular eclipse at noon..	11 11
in $223^\circ 25'$ east longitude from Ferro.	
15 12 south latitude.	
End of the central eclipse on the earth..................	13 6
in $280^\circ 47'$ east longitude from Ferro.	
11 51 north latitude.	
End of the eclipse on the earth in general.............	14 13
in $264^\circ 19'$ east longitude from Ferro.	
11 10 north latitude.	

Visible in the Pacific, the western parts of America, and the Isthmus of Panama, as also in the eastern parts of New Holland.

Paramatta,	Beginning	$17^h 48'$	app. time of Paramatta.
	End..................	19 40	
	Magnitude	4	inches.
Mexico,	Beginning..........	4 28	app. time of Mexico.
	Middle..............	5 35	at sun-set.
	Magnitude..........	8·7	inches.

Transit of Mercury over the Sun's Disc, May 4 *and* 5, 1832.

		App. time of Berlin.
Entrance of the centre of Mercury on the sun's disc, seen from the centre of the earth,	May 4,	$21^h 58' 20''$
Shortest distance $8' 0''{\cdot}5$	May 5,	1 22 53
☿'s centre leaving the ☉'s disc.........	— –	4 47 8

At the moments of the centre of ☿ entering upon, and leaving the sun's disc the sun is respectively in the zenith of the places whose geographical position is

$61^\circ 28'$	east of Ferro.........	$16^\circ 18'$	north latitude.
319 16	————	16 23	———

All Europe and the greater portion of Africa see the whole duration, Asia only the beginning, America only the end of this transit.

In order to calculate the effect of parallax on the time of beginning and termination for any place in latitude ϕ, and east longitude from Ferro l, we may find the angular distance of this place from a point whose

east longitude from Ferro .... $= \lambda = 166^\circ\ 18'{\cdot}8$
north latitude.......................... $= \beta = +\ 53\ \ 57{\cdot}0$

by this formula:

$$\cos\zeta = \sin\beta\sin\phi + \cos\beta\cos\phi\cos(\lambda - l);$$

then the actual entrance of ☿'s centre on the ☉'s disc will take place at the apparent time of the place of observation:

$$\text{May 4.}\quad 19^h\ 54'\ 6'' + l - 118''{\cdot}8\cos\zeta.$$

For

For the termination of the transit put

$$\lambda' = 229° \ 1'{\cdot}5 \quad \beta' = +\ 2° \ 56'{\cdot}0; \text{ and calculate}$$

$$\cos \zeta' = \sin \beta' \sin \phi + \cos \beta' \cos \phi \cos (\lambda' - l);$$

and ☿'s centre will leave the ☉'s disc

$$\text{May } 5. \quad 2^h \ 42' \ 55'' + l + 118''{\cdot}8 \cos \zeta'$$

apparent time of the place of observation. The contacts of the limbs will respectively take place about 1′ 33″ sooner or later.

For Berlin, where

$$\phi = 52° \ 31'{\cdot}2, \quad l = 31° \ \ 3'{\cdot}5,$$

we find $\cos \zeta = +0{\cdot}3873 \quad \cos \zeta = -0{\cdot}5375$

☿'s centre enters on	the ☉'s disc	$21^h \ 57' \ 34''$	App. time of Berlin.
— — leaves		4 46 5	

The entrance on the ☉'s disc takes place 32° eastward, the parting from the ☉'s disc 87° westward, of the northernmost point of the ☉'s disc.

The duration of the transit of each second in arc of ☿'s diameter over the ☉'s disc is = 17″·3 in time.

Solar Eclipse, July 27, 1832.

	App. time of Berlin.
Beginning on the earth in general........................	$0^h \ 11'$
in 294° 29′ east longitude from Ferro.	
10 59 north latitude.	
Beginning of the total eclipse on the earth	1 6
in 280° 5′ east longitude from Ferro.	
12 53 north latitude.	
Total eclipse at noon	2 47
in 349° 23′ east longitude from Ferro.	
24 34·5 north latitude.	
End of the total eclipse on the earth.....................	4 31
in 54° 24′ east longitude from Ferro.	
2 47 south latitude.	
End of the eclipse on the earth in general	5 26
in 38° 11′ east longitude from Ferro.	
4 23 south latitude.	

The eastern limit of visibility of this eclipse runs through Europe, from the north coast of Ireland above Greenwich, along the boundary of Germany and France, to the coast of Dalmatia. To a country to the westward it will be visible; in Germany it will not be visible. The western limit embraces the greatest part of North America and a great part of South

South America. Almost all Africa may see it; but only the west coast of Asia.

For calculating the beginning and end of the eclipse for places in Europe the following formulæ may be made use of:

Let the longitude of a place from Berlin $= l$ (negative if west), the corrected latitude $= \varphi$, and let the following values be calculated:

$$u = 1{\cdot}8816 \cos\phi \sin(45^\circ + l)$$
$$v = 1{\cdot}7773 \sin\phi - 0{\cdot}6177 \cos\varphi \cos(45^\circ + l)$$
$$u' = 0{\cdot}4926 \cos\phi \cos(45^\circ + l)$$
$$v' = 0{\cdot}1617 \cos\phi \sin(45^\circ + l)$$

$$m \sin \mathrm{M} = +0{\cdot}2404 - u$$
$$m \cos \mathrm{M} = +0{\cdot}1431 - v$$
$$n \sin \mathrm{N} = +1{\cdot}0862 - u'$$
$$n \cos \mathrm{N} = +0{\cdot}1554 - v'$$
$$m \sin(\mathrm{M}-\mathrm{N}) = \cos\psi,$$

where ψ must always be taken positive and less than 180°. The time of beginning and termination of the eclipse will then respectively be

$$3^h + l - \frac{m}{n}\cos(\mathrm{M}-\mathrm{N}) \mp \frac{\sin\psi}{n}$$

apparent time of the place of observation expressed in hours and parts of an hour; the upper sign referring to the beginning, the lower to the termination of the eclipse. The angles which, at those two moments the respective radius of the sun's disc at the point of contact forms with the horary circle of the sun's centre, counted from north through east to 360°, will be respectively $\mathrm{Q} = 90^\circ + \mathrm{N} \pm \psi$,

and the magnitude of the eclipse in inches will be

$$= 25 \,.\, \sin \tfrac{1}{2}\psi^2.$$

Elements of Solar Eclipses.

Apparent time of Berlin.

	February 1.	July 27.
●	11ʰ 9′ 25″·8	2ʰ 49′ 2″·7
Longitude of ☾ and ☉...........	312° 8′ 45″·2	124° 26′ 47″·5
Horary motion of ☾ in longitude	30 24 ·8	37 51 ·1
——————— ☉ ———	2 32 ·2	2 23 ·5
☾'s latitude........................	+0 1 54 ·1	+0 5 43 ·9
Horary motion of ☾ in latitude	− 2 49 ·1	+ 3 30 ·5
☾'s parallax	54 43 ·8	61 14 ·4
☉'s parallax	8 ·7	8 ·5
☾'s semidiameter..................	14 54 ·8	16 41 ·3
●'s semidiameter	16 15 ·3	15 47 ·0

Elements

Elements of the Transit of Mercury.

Apparent time of Berlin.

	Beginning.	End.
May 4 and 5	$22^h\ 0'\ 0''$	$4^h 48'\ 0''$
☉'s right ascension	$42°21'\ 50''·0$	$42°38'\ 12''·9$
☿ ————————	42 30 34 ·1	42 21 38 ·3
Horary motion of ☉ in Æ	+ 2 24 ·5	+ 2 24 ·5
———————— ☿ ———	— 1 18 ·7	— 1 18 ·8
☉'s northern declination	16 18 1 ·0	16 22 50 ·6
☿'s ————————	16 31 23 ·1	16 23 36 ·9
☉'s horary motion in declination	+ 42 ·7	+ 42 ·6
☿'s ————————————	— 1 8 ·4	— 1 8 ·6
☉'s semidiameter	15 52 ·4	15 52 ·3
☿'s ———————— [distance 1 = 3″]	5 ·37	5 ·37
☉'s parallax	8 ·5	8 ·5
☿'s ———	15 ·35	15 ·38

IV. *Of the Conditions of Life. By the Rev.* Patrick Keith, *F.L.S.**

WHAT is life?—The great variety of definitions by which physiologists have attempted to exhibit an idea of life, shows that it is no easy task to do so correctly. The subtile and untangible character of the subject to be defined is doubtless the grand cause of the difficulty. Bichât, a French physiologist of great celebrity, defined it as follows: "La vie est l'ensemble des fonctions qui resistent à la mort †,"—Life is the totality of the functions that resist death. It is a trait from the pencil of a great master, but it is by much too indefinite to exhibit a distinct view of the subject. Functions seem to be rather the result of life, than to be life itself. But what is the amount of their resistance? for death finally overcomes them; and of what class of bodies are they predicable?—Richerand defined it thus: "La vie est une collection de phénomènes qui succedent pendant un temps limité dans les corps organisés ‡,"—Life is a collection of phænomena that occur during a limited period in organized structures. But the boundaries of this limited period are left undefined, and must consequently be supplied by the imagination of the reader. They may include even the phænomena of death, for anything that the definition shows to the contrary. Mr. Lawrence's

* Communicated by the Author.

† *Recherches Physiologiques.*

‡ *Traité de Physiologie.*

definition

definition is very brief. It is as follows: "Life is the active state of the animal structure*." This evidently excludes the torpid state. It excludes also vegetables, which it might indeed be made to embrace by the insertion of a single word. But if life may exist even in a state of rest, surely it cannot be well defined, merely by calling it a state of action.

A writer, who regards the above definitions as savouring too much of materialism, not to say atheism, gives us another definition,—the briefest of all: "Life is inherent activity†." This, it must be acknowledged, is scanty enough: but it is abundantly comprehensive; for it includes everything of which inherent activity can be predicated, be it mind, or be it matter. Yet this is going a great deal further than its author intended, seeing that it is an approach to the materialism of which he accuses others. There is an inherent activity in the movements of the Aurora borealis, the merry dancers of the North; but it is not life. There is an inherent activity in the vivid coruscations that dart across the sky, and illuminate the loaded atmosphere in a night of electrical tempest; but it is not life. There is an inherent activity in the cause that occasions the eruption of a burning mountain,—that subterraneous artillery which melts the solid and primæval rocks, and upheaves them in floods of liquid lava; but it is not life. Yet according to the definition we ought to say that it is life, because it is a manifestation of inherent activity. Now inherent activity is not even an entity, but an abstraction. It is not life, but a property of life. It is not peculiar to living bodies, but is possessed by many bodies that are thought to be inert. What are chemical, magnetical, and electrical repulsions and attractions, if they are not examples of inherent activity?

It would be presumption in me to attempt to do that which the above distinguished physiologists, or their more orthodox criticizers, have failed to do; namely, to exhibit a correct idea of life, by the selection of a single trait. But as their

* Lawrence On Physiology. 52.

† Remarks on Scepticism; by the Rev. T. Rennel. 1819.

[Mr. Rennel's work, not long ago, made some noise in the world. We believe, however, that this writer is destined to find his place in the temple of fame among those worthies who in former times insinuated charges of atheism against Newton and Locke; and we trust that the same freedom will be claimed for the philosophy of mind, which has of late been ably asserted, by Professor Sedgwick and others, for Geology and Zoology. Surely nothing can be less calculated to confirm the religious belief of mankind than attempts to persuade them that every discovery or new view in philosophy is hostile to religion. Can those be justly charged with irreligion who refuse to assign arbitrary limits to the power of the Deity, as to the properties which he may confer on matter?

We shall have to notice a recent instance of misrepresentation similar to that to which we here allude, in our next.—Edit.]

 failure

failure seems to be attributable, chiefly, to an unnecessary effort at brevity,—*brevis esse laboro, obscurus fio*,—perhaps it might be worth our while to try the effect of a fuller enumeration of particulars. What we lose in point and neatness, we may gain in perspicuity; and upon this principle I submit the definition that follows:—Life is that attribute or energy of organized structures which renders them susceptible to impressions, and enables them to discharge functions. It is real, or it is potential:—real, if the energy is in operation; as in the case of an animal in motion, or of a plant protruding its buds and blossoms;—potential, if the energy is dormant, as in the case of an egg not hatched; or of a seed not sown; or, as in the case of the hybernation, whether of plants or animals. Life originates in precedent life, and terminates in subsequent death, which is an extinction of all vital functions, and of all possibility of vital functions.—Taking this definition, with its illustration, as our text, we proceed to remark that life in the exhibition of its phænomena always presupposes the existence of certain peculiar conditions, previous, concomitant, or consequent, without which it has never been known to manifest itself, and of which the most essential are the following,—parentage, organization, aliment, aëration, temperature, death.

Parentage.—There was a time in which philosophers believed in what was called the doctrine of equivocal generation —that is generation springing from a fortuitous concourse of atoms having an appetency to combine themselves into living forms. This doctrine was taught and maintained by the most celebrated philosophers of antiquity. Plants were regarded by Diogenes as being generated from a mixture of earth and putrid water; the water acting upon the earth, and moulding it into form *. Plants whose seeds are not apparent were regarded by Theophrastus as being propagated by spontaneous generation, because some tribes of animals were thought to be so propagated †; and parasitical plants were regarded as springing from some corrupted matter generated on the tree producing them ‡. The poets of antiquity had the same fictions. Ovid replenishes his post-diluvian world with animals that sprang up out of the earth *sponte sua*, excited by heat and moisture §. The philosophy of the dark ages was not likely to correct the errors of antiquity; and when a better philosophy was introduced, at the time of the revival of learning, it could not be expected to correct them all at once. Even Bacon, the great reformer of philosophical methods, still gave his sanction to the doctrine of equivocal generation, as

* *Theophr.* Περι Φυτων ἱστοριας, lib. iii.

† Περι Φυτων αιτιων, lib. i.

‡ *Ibid.* lib. v.

§ *Metamorph.* lib. i.

as may be seen by perusing his *Sylva Sylvarum.* The mosses that grow on trees he regards as being nothing more than a sort of excretion which the tree cannot assimilate; and the misletoe he represents as being produced, not from seed, but merely from superabundance of nourishment.

Yet the truth of the doctrine began to be at last suspected, and subjected to the test of observation and experiment. It was a scrutiny which it could not stand, and beneath which it fell refuted. The credit of the refutation is due partly to Harvey, who contended that all animals spring from an egg deposited by a parent,—*omne animal ex ovo;* and partly to Francisco Redi, an Italian philosopher and physician, whose experiments are well known, together with their result; namely, that there is no such thing as a generation of insects from putrefaction. Similar investigations were applied to the vegetable kingdom by Malpighi and others, with similar results, demonstrating that all plants spring from seed, the produce of a parent,—*omnis planta e semine.* In short the doctrine of equivocal generation came into universal disrepute; and the stories of showers of frogs that fell from the clouds, and of armies of insects engendered by the east wind, and wafted on its wings, together with the marvellous account of a plane tree that sprang up spontaneously out of a brazen tripod, as related by Theophrastus, were no longer credited.

Such was the triumph of truth over error. Yet the very progress of the science that achieved it gave rise to new doubts. In the advance of microscopical discovery, a new world was laid open to the view of man, namely, that of the *animalcula infusoria* and *spermatica.* The most successful of the earlier operators in this department were Leuwenhoeck, Needham, and Swammerdam. Leuwenhoeck estimated the size of the smallest of these minute animalcules, and found that upwards of 1,000,000 of them might be contained in a space not larger than that occupied by a grain of coarse sand*. Concerning their origin every philosopher had his own opinion. Some regarded them as being generated by parents of the species, rather from analogy, than from any direct proof, which, in objects so minute, it must be next to impossible to obtain. Buffon regarded them as being, not the product of generation, not germs or embryos, not either animals or vegetables, but merely organic and moving particles proper to compose a living being.—If this were really the case, the species or varieties of animalcula would be interminable, as there would be no end to new combinations of organic particles, and no certainty of finding tomorrow the species you may have met with

* Phil. Trans. Abridged; vol. ii. 377. and vol. iii. 203.

with today. But the contrary of all this is the fact. The species are not interminable, but circumscribed, and in some of them the mode of propagation has been actually ascertained. But animalcules have been found in infusions that have been boiled, roasted, and even subjected to the heat of a blowpipe; and this has been regarded as a proof that they have not proceeded from anything possessing life. Yet some species of seeds will survive even the action of boiling, and their germs, as we may suppose, are not less vivacious than themselves; and if the infusions in question were deprived of everything vital, whence came the animalcules?—They must have come from without. They must have penetrated the containing vessels; —a fact which cannot be admitted without due evidence. Fray affirms that he found animalcules in mineral mixtures *; and the same doctrine has been again advanced by Brown†, who finds them in rock, glass, ashes, soot, when ground to an impalpable powder and mixed with a little water. But as this question may be said to be yet *sub judice*, we will adopt the proposition of Cuvier, and say with him, "La vie ne nait que de la vie‡,"—Life originates only in life.

This proposition has been a good deal carped at, unfairly, as we think, by Barclay§, who disapproves of the doctrine which it contains altogether, and asks whether the first individual, or the first pair of any species, could have come into existence in this manner?—The answer is ready. Cuvier's object was not that of tracing phænomena to their first causes, which may lie concealed for ever from human view, but merely to such causes as fall within the sphere of human observation, and are cognizable by human means. This is philosophy; this is physiology. The study of a first cause is religion; and our knowledge of it is derived, chiefly, from revelation. We do not say that it must necessarily be excluded from physiological research; but if the individual inquirer chooses to exclude it for the purpose of keeping separate two subjects that are perfectly distinct, he is not therefore to be regarded as an atheist. What light has Dr. Ure thrown on geology by his boasted introduction of the agency of a first cause?—But, says Barclay, what are we to make of the *animalcula infusoria?* It has not been proved that they are the product of generation;

* *Essai sur l'Origine des Corps organisés:* Barclay.

† [Justice to Mr. Brown requires from us a remark on this point. It will be found, on perusing that gentleman's papers on Active Molecules (Phil. Mag. and Annals, N. S., vol. iv. p. 161, vi. p. 161), that he nowhere ascribes *animal life* to them, as the cause of their motion, and consequently ought not to be stated as regarding them to be *animalcula*. In the latter paper, indeed, he has expressly denied that he considers the active molecules to be animated.—Edit.]

‡ *Leçons d'Anat. Compar.:* Barclay. § On Life and Organization. 318.

and

and yet they are evidently endowed with life.—To this we will reply by the following question. Has it been proved that they are not the product of generation?—If the higher orders of animals, the animals with which we are best acquainted, are evidently the product of generation, ought we not to conclude from analogy, that other and inferior orders of animals, with which we are less acquainted, are the product of generation also? or at the least of some process analogous to it, and leading to a similar issue. On this ground we rest satisfied of the legitimacy of Cuvier's conclusion.

Organization.—A very general and very good division of the bodies existing in nature, is that by which they are distributed into two primary classes of organized and unorganized productions. If we suppose a gradation by which all natural bodies are placed according to their rank in the scale of being, the unorganized substances will be found at the bottom. They exhibit no indications of life, no susceptibility to impressions, no sympathy of parts, no functions. Still they possess a definite number of properties by which they are readily characterized. Their properties are physical or chemical;—gravity, elasticity, mobility, affinities, attractions, repulsions. They display also a gradation among themselves. Some of them are found merely in shapeless lumps that accident seems to have thrown together, and that accident may again disperse;—masses of rock, masses of minerals. Others are found to present themselves in regular and symmetrical forms, whether individually or in the aggregate;—crystals, masses of crystals. If we regard the fabric of the earth which we inhabit, we find that it is moulded into an immense and globular mass; but it is destitute of all organization, as are the fluids with which it is watered, and the gases with which it is surrounded. The same remark may be extended, as we presume, to the heavenly bodies also;—the sun, the stars, the planets, and their satellites.

Organized bodies form the second class. They stand higher in the scale of being, and are endowed with nobler properties. They are the sole receptacles of life, which has never yet displayed itself except in such fabrics. They consist, in their living state, partly of solids, and partly of fluids in motion. The fluids are the materials out of which the solids have been formed,—chyle, blood,—sap, proper juice; or they are secretions, or exhalations, or excrements coming from the solids;—bile, urine, perspirable matter,—gum, nectar, perspirable matter. In the aggregate they form a fabric which is composed of a definite system of individual fabrics or organs, which constitute in their assemblage an individual whole;—a plant,

plant, an animal. An organ is a fabric adapted by its structure to the performance of a function;—a hand, a foot, a leaf, —prehension, progression, aëration. Bichât has remarked that the organs destined to the higher functions of the higher orders of animals—the organs by which they communicate with the external world—are more symmetrical in their form than other organs, and many of them double, as the eyes, the ears, the hands, the feet; or divisible into two corresponding halves, with a manifest medial line,—as the brain, the tongue. The higher the function the greater the symmetry of the organ *. We may extend this remark to vegetables also. It will not be found to apply so generally, nor in the same degree; but it is easy to discern traces of the fact. The leaves and petals are among the most important of all vegetable organs, and they show this peculiarity very conspicuously. They are divisible into an anterior and posterior surface; and into a right and left side separated by the intervention of a midrib. The interior organs of the flower may be regarded as divisible into two equal and similar halves; the spongiolæ of the radicles may be regarded in the same light; and perhaps the beautifully twisted form of the spiral threads should be regarded also as an example of the symmetry in question. An assemblage of several organs all concurring to the production of a single result constitutes an apparatus,—the visual apparatus, the digestive apparatus, the lacteal apparatus. An assemblage of organs possessing the same or a similar structure constitutes a system,—the vascular system, the osseous system, the nervous system. The immediate constituents of organs are tissues,—the cellular tissue, or the fibrous tissue.

Of all living bodies whether plants or animals, the principal mass is composed of the cellular tissue. It enters into the composition of almost every organ, and binds and cements together the fibres that pervade it. Particularly, it forms the principal mass of succulent plants, and a notable portion of many parts of woody plants. It abounds in succulent fruits, and in the lobes of all seeds. It consists of clusters of little cells or vesicles containing an inclosed fluid, which Grew compared in their aggregate aspect to the bubbles formed upon the surface of liquors in a state of fermentation.—The fœtus seems a homogeneous mass of cellular tissue filled with a gelatinous fluid. As the organs begin to show themselves this mass becomes more condensed. As the bulk of the organs increases, the proportional bulk of the tissue diminishes. It is the receptacle of lymph and of fat, and is at all periods of

* *Recherches Physiologiques*, par Xav. Bichât. 8.

life

life that on which depends the plumpness or *embonpoint* of the individual*.

Of all living bodies whether plants or animals, a notable portion is composed of the fibrous tissue. The fibres are arranged in groups or bundles passing longitudinally throughout the whole extent of the organ, as in the slip of *Aspidium Filix-mas;* or in the nerves, muscles, and tendons of the animal fabric. When viewed superficially, a group appears to be merely an individual fibre; but when inspected minutely, and under the microscope, it proves to be made up of fibres smaller and minuter still, firmly cemented together and forming in the aggregate a strong and elastic thread, but capable of being split into a number of component fibriles, till at last they become so fine that you can divide them no longer. But in some organs they are arranged in thin plates or laminæ, as in the net-work of the cortical and woody layers of plants; and in the composition of the sclerotica and periosteum of animals †.

Every organ is invested, and if admitting it, lined, with an envelope of fibrous or of cellular tissue; and every living individual is enveloped with a covering of bark or of skin, or at the least with a fine epidermis. If the tissues are themselves examined with a view to ascertain the elements of their own composition, they will be found to consist of fine films or fibriles, which seem to be themselves composed of multitudes of minute and gelatinous globules closely compacted together, and distinguishable only by the microscope. Their diameter is represented as not exceeding the $\frac{1}{8000}$th part of an inch; but their existence is by some doubted. Beyond this, the analysis of the dissector cannot go. Here his anatomy ends. If he proceeds to chemical analysis, he will find that the proximate principles of the animal solids are chiefly albumen, fibrin, gelatin; the remote principles being azote, oxygen, hydrogen, carbon, with the azote predominating ‡.

Of the vegetable solids he will find that the proximate principles are chiefly albumen, fibrin, gluten, sugar, gum, extract; the remote principles being carbon, hydrogen, oxygen, with the carbon predominating §. When the proximate principles of animals, albumen, fibrin, gelatin, are converted artificially from a fluid to a solid state, as by the action of heat or of other chemical agents, multitudes of minute globules, similar to those of the blood, are said to be developed in the

* *Anatomie Générale,* par M. Bichât. 98.

† *Anat. Générale,* tom. ii. 251.

‡ Magendie, by Milligan. 10.

§ Davy's Agricultural Lectures.

mass.

mass*. It is a presumption in favour of the globulous structure of ultimate, living tissue.

Do the vital powers reside in the fluids, or in the solids, or in both? Some physiologists would confine them to the solids. But if the solids originate in the fluids, how can the fluids themselves be destitute of vital powers? They would thus present the singular anomaly of communicating that which they do not possess. Mr. J. Hunter believed in the vitality of fluids; or, at the least, in the vitality of the blood. But Blumenbach can see no ground for adopting this opinion. If you grant vitality to the blood because it is the material out of which the living solids are formed, as well, says he, may you grant it to water because the Nympheæ and many other remarkable plants are nourished by it †. We do not think that the case is fairly put. Blood is an elaborated fluid fit for immediate assimilation, and the vegetable fluid corresponding to it is not water, but proper juice.

Bichât is also of opinion that fluids do not possess vitality, because they are incapable of contraction and of sensibility. Fluids he regards as merely passive, and solids only as active ‡. But fluids are stimulants, or exciters at least, and thus they act upon solids. The blood stimulates the heart to action, and ardent spirits stimulate the brain. After all, Bichât admits that the fluids begin to acquire animalization and vital properties in the course of their elaboration in the system. The chyle is more animalized than the alimentary mass; the blood than the chyle. This is granting enough to sanction the vitality of some fluids; and the vitality of all fluids is not contended for. But why should we doubt the possibility of endowing a fluid with vitality; or in what respect is it more easy to conceive a solid endowed with it than a fluid? The rudiments of the solids exist already in the fluids, that is, in the fluids elaborated for nutrition; so that the former can be nothing else than a more compact aggregation of the minute and semi-organized globules of the latter, effected by the agency of the living powers of the plant or animal in some specific and determinate manner. The fluids of the human body are represented by Bichât as being to the solids in the ratio of 9 to 1; and the fluids of vegetables as being to the solids in a greater ratio still. Many of them contain globules of a regular figure and magnitude, particularly the blood, lymph, and chyle; and the spermatic fluid contains millions of animalcules. The fluids yield by chemical analysis the same principles as the solids §.

[To be continued.]

* Edwards *De l'Influence des Agens physiques sur la Vie.*

† Blumenbach's Physiology, by Elliotson. 21.

‡ *Anat. Génér.* 24.

§ Magendie, by Milligan. 14.

V. *Proceed-*

V. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 5.—A PAPER was read, "On the effect of Water, raised to Temperatures moderately higher than that of the Atmosphere, upon Batrachian Reptiles." By Marshall Hall, M.D., &c.

Dr. Edwards had found, by a series of experiments, that the batrachian reptiles, when immersed in hot water, live for a shorter time in proportion as the temperature of the water is higher; and that at 108° of Fahrenheit they die almost instantaneously. The author of the present paper observes, that the extinction of life in these cases is owing to a cause of a more immediately destructive agency than the mere suspension of respiration: he finds that if only the head of the animal is placed under water of 120°, the animal struggles, but soon ceases to move; but if the spine as well as the limbs be immersed, convulsions supervene, and the muscles become rigid: in both cases the action of the heart continues. If one of the limbs, which after the extinction of sensibility still remains flexible, be separated from the body, and placed in water of 120°, its muscles contract and become rigid; this effect taking place first in the superficial, and next in the deep-seated muscles. When the nerve, separated from the other parts, was alone placed in hot water, the muscles were not affected: and when the muscles had been made to contract by hot water, they were no longer capable of being affected by irritations applied to the nerve. The heart removed from the body, and placed in hot water, gradually contracted and remained rigid. Hence the author concludes that the death of the animal, when occasioned by the sudden application of heat to the surface, is not owing to asphyxia, but to a positive agency, destroying the functions of the nervous and muscular systems; the muscles of involuntary motion being affected in like manner with those of voluntary motion.

A paper was read, entitled an "Account of a new mode of propelling Vessels." By Mr. Wm. Hale. Communicated by Richard Penn, Esq. F.R.S.

The author ascribes the want of success which has hitherto attended all attempts to propel vessels by a discharge of water from the stern, to the injudicious plan of the apparatus employed, and not to any defect in the principle itself: for he considers that the reaction upon the vessel from which a volume of water is thrown, depends in no degree on the resistance it meets with from the medium into which it is ejected, but simply upon the momentum given to the mass. The author proposes to accomplish the object of propelling water by means of an instrument having the form of an eccentric curve, resembling the spiral of Archimedes, made to revolve on an axis. The resistance offered to the water in which it is immersed results from the different distances of the two ends of the spiral propeller from the axis. This propeller acts in a box having also a somewhat spiral form, and the space between the two ends of the spiral, after describing one turn, is open to allow of the exit of the water driven out by the propeller. The bottom of the box has a circular aperture, of

which the radius is equal to the distance of the shorter end of the propeller from the axis. The water within this circle meets with no resistance until it arrives at the line joining the two extremities of the propeller, when it is immediately acted upon by the eccentric curved surface of the propeller.

A paper was read, entitled, "Additional thoughts on the use of the Ganglions in furnishing Electricity for the production of Animal Secretions." By Sir Everard Home, Bart., F.R.S.

The author considering animal heat as depending on the ganglions, infers from the analogy of the structure of the abdominal ganglia with the electrical organs of fishes, that animal heat arises from the electricity supplied by these ganglions.

May 12.—A paper was read, "On a peculiar class of Acoustical Figures; and on certain forms assumed by groups of particles upon vibrating elastic surfaces." By Michael Faraday, Esq., F.R.S., M.R.I., Corresponding Member of the Royal Academy of Sciences of Paris, &c.

When elastic plates on which sand has been strewed are thrown into sonorous vibrations, the grains of sand arrange themselves in lines which indicate the quiescent parts of the plate, and have been called the nodal lines. This fact was discovered by Chladni, who also observed that the minute shavings cut by the edge of a glass plate from the hairs of the violin bow employed to produce the vibration, collected together on those parts of the plate that were most violently agitated, that is, at the middle of the lines of oscillation, or portions into which the plate is divided by the nodal lines. The same phænomenon is exhibited by lycopodium, or any other very light and finely divided powder. This subject was investigated by M. Savart, who, in a paper read to the Royal Academy of Sciences at Paris in the year 1817, endeavoured to account for this latter class of phænomena by deducing from the primary divisions of the parts of vibrating bodies, certain secondary modes of division, comprising parts that remain horizontal during every stage of the vibration, and which therefore may admit of the settlement there of light powders, while heavier powders can be stationary only at the points of absolute rest.

This explanation not appearing to the author to be satisfactory, he made a great number of experiments, which are detailed at length in the present paper, showing that the immediate cause of these motions exists in the surrounding medium, and is to be found in the currents arising from the mechanical action of the plate, while vibrating upon that portion of the medium which is in contact with the plate. These currents are directed from the quiescent lines towards those parts where the oscillation is the greatest, and meeting from opposite sides at these central points, thence proceed perpendicularly from the vibrating surface to a certain distance; and finally, receding from each other, return again in a direction towards the nodal lines. The combination of these motions constitutes vortices carrying with them any light particles which may lie in the way of the currents. While in motion, the powders sustained by these vortices appear in the form of clouds, the particles of which have

have among themselves an intestine motion of revolution, rising in the centre of the heap, and rolling down again on the outer sides. The powders are collected in the same situations on the vibrating plate, although the plate may be considerably inclined to the horizon, and remain there even when the inclination is so great as to prevent grains of sand from resting on the nodal lines. A piece of gold leaf laid upon the plate was raised up in the form of a blister at that part which corresponded with the centre of the clouds, even to the height of one-twelfth of an inch.

On attaching small pieces of card to different parts of the surface of the vibrating plate, the currents of air are modified in various ways, as shown by the different positions of the clouds, and the production of partial accumulations of the powders. When a tuning-fork is made to vibrate, and a little powder of lycopodium is sprinkled over it, the powder collects into clouds on the middle of the upper surface, and also forms heaps along its sides, exhibiting in a striking manner the intestine revolution of their particles. These effects are also well illustrated by vibrating membranes; for which purpose a piece of parchment was stretched, and tied while moist over the mouth of a funnel, and made to vibrate by means of a horse-hair, having a knot at the end, passed through a hole in the centre of the parchment; the hair being drawn between the finger and thumb, to which a little powdered rosin was previously applied. The phænomena were still more conspicuous when the parchment was made to vibrate under a glass plate held near it. When the interval between the membrane and the glass plate was very small, the whole of the powder was sometimes blown out at the edge, in consequence of the vibrating membrane acting as a bellows.

Reasoning from the theory which the author had framed in explanation of these phænomena, he conceived that if the currents were weakened by placing the apparatus in rarefied air, they would no longer be capable of sustaining the light powders, which would then be collected, like the heavy powders in air, at the nodal lines. In a denser medium, such as water, the reverse should happen; the heavy powders should be carried along by the more powerful currents then produced, and would accumulate in the vibrating parts. All these conclusions were found to be fully verified by actual experiment.

May 19.—A paper was read, entitled, "A Table facilitating the Computations relative to Suspension Bridges." By Davies Gilbert, Esq. V.P. R.S.

The table here communicated is supplementary to those accompanying the paper "On the Mathematical Theory of Suspension Bridges," which was published in the Philosophical Transactions for 1826, and is deduced from the first of the tables there given; but admits of a far more ready application than the former to all cases of practical investigation. It consists of five columns, exhibiting respectively the deflections or versed sines of the curve; the lengths of the chains; the tension at the middle points, or apices of the curve; the tensions at the extremities; and the angles made by the chains with the horizon at the extremities.

 A paper

A paper was read, entitled, "Researches in Physical Astronomy." By J. W. Lubbock, Esq. V.P. and Treasurer of the Royal Society.

The first part of this paper relates to the theory of the moon. The method of solution pursued by Clairaut consisted in the integration of differential equations, in which the true longitude of the moon is the independent variable: the time is then obtained in terms of the true longitude; and by the reversion of series, the longitude afterwards obtained in terms of the time. This method is the one adopted by Mayer, Laplace, and Damoiseau. The author has been led, by reflecting on the difficulties of this problem, to believe that the integration of the differential equations in which the time is the independent variable would be at least as easy as the former process; and it would possess the advantage of employing the same system of equations for the moon as for the planets. The lunar theory proposed by the author, and developed in this paper, is an extension of the equations given in his former Researches in Physical Astronomy, already published in the Philosophical Transactions; by including those terms, which, in consequence of the great eccentricity of the moon's orbit, are sensible; and by suppressing those which are insensible from the great distance of the sun, the disturbing body. He has not yet attempted to obtain numerical results, but proposes at some future time to engage in their computation.

In the second part of the paper, he investigates the precession of the equinoxes, on the supposition that the earth revolves in a resisting medium; an investigation which may also be considered as a sequel to the author's last paper on Physical Astronomy. The effects of the resistance of such a medium is to increase the latitude of the axis of rotation (reckoned from the equator of the figure) till it reaches 90°. Such is now the condition of the axis of the earth: but as the chances are infinitely great against this having been its original position, may not its attainment of this position be ascribed to the resistance of a medium of small density acting for a great length of time,—a supposition which may account for many geological indications of changes having taken place in the climates of the earth? The operation of such a cause would be also sensible in the case of comets: and the accuracy with which the eccentricity of the Halleian comet of 1759 is known, would appear to afford a favourable opportunity of verifying this hypothesis.

A paper was read, entitled, "An Account of the Construction and Verification of the Imperial Standard Yard for the Royal Society." By Captain Henry Kater, F.R.S.

The scale of the standard, of which an account is given in this paper, is constructed in the manner described in the Philosophical Transactions for 1830. The support is of brass 40 inches long, 17.5 inches wide, and 0.6 of an inch in thickness. A brass plate seven-hundredths of an inch thick was made to slide freely upon the support in a dove-tail groove formed by two side plates, and was then fixed to the support by a screw passing through its middle. This plate carries the divisions, which are fine dots upon gold discs let

let into the brass; the scale is divided into inches, and there is one inch to the left of zero, which is subdivided into tenths. The scale is the work of Mr. Dollond. The paper is concluded by an account of the precautions which were taken to ensure the accuracy of the plane surface on which the bar rested, while the comparisons were made with the microscopic apparatus described in the Philosophical Transactions for 1821. The results are given in a table.

A paper was read, entitled, "An Experimental Examination of the Blood found in the Vena Portæ." By James Thackeray, M.D. Communicated by Sir Astley Cooper, Bart. V.P.R.S.

The author, in the course of an inquiry into the properties of the blood, was led to notice some peculiarities in the contents of the vena portæ, and to investigate this subject more minutely. The results of the experiments which he made for this purpose are chiefly the following. The blood contained in the vena portæ is darker than that of the other veins, inclining more to a ruddy hue than to the Modena red. Being less homogeneous, it has the appearance of being less perfectly elaborated. Its specific gravity was found to be very variable, but it is in general less than ordinary venous blood. It coagulates much more quickly, and contains a larger proportion of serum, but a much smaller proportion of albumen, than blood taken from other veins. The serum obtained from it is redder than common serum, in consequence of its retaining much of the colouring matter of the blood: it has also a greater specific gravity, and yields, on exsiccation, a greater weight of solid matter. On the application of heat, it concretes more quickly, but much less completely, than blood from the jugular vein; which peculiarities are attributed by the author to the different state and imperfect formation of the albumen contained in it. The crassamentum of the blood from the vena portæ does not expel its serum so fully as blood from other vessels; but it remains a soft mass, unless artificial means be employed, and it yields a considerably smaller quantity of fibrin.

GEOLOGICAL SOCIETY.

May 11th, and 25th.—At the meetings held on these evenings, a paper was read, entitled "A Sketch of the principal Secondary and Tertiary Formations of Germany,—by Roderick Impey Murchison, Esq., Pres. G.S., F.R.S. &c."

This communication is derived from a series of memoranda which the author has extracted from note books, written as he passed through various parts of Germany in the last three years; and he presents it to the Geological Society in the hope, that it may rouse the attention of his countrymen to the increasing geological interest of that country, and to the various valuable native publications which describe its subdivisions. He endeavours to point out, in ascending order, all the German formations from the surface of the carboniferous rocks up to the newest tertiary deposits, showing, as far as is possible, their analogies and discrepancies when compared with those of England;

England; and entering into detail on such points only as fell directly under his own observation. He refers for an account of places not visited by himself, to the general work of M. Boué, and to various local authorities.

In citing, with much praise, the recently published maps and sections of Hoffmann on North-Western Germany, the English inquirer is cautioned against the general application, in Germany, of that part of the table of superposition, in which the coal measures are designated as some beds, subordinate to a vast thickness, 3000 or 4000 feet, of red sandstone and conglomerate, the whole of which are grouped by Hoffmann under the one term of *rothe-todte liegende.* It is shown, on the contrary, that, however well this classification may apply to a small part of Germany, it is by no means the rule in the N.E. part of Bavaria, in Bohemia, and Westphalia; in all of which countries there are successions in the carboniferous series, very similar to those in England, accompanied with large expansions of mountain and transition limestones. The author, therefore, adopts that view of Professor Sedgwick which restricts the name of *rothe-todte-liegende* to those sandstones and conglomerates which surmount the carboniferous series, and separate it from the *kupfer-schiefer* and magnesian limestone.

In describing the *kupfer-schiefer* and overlying limestones, *zechstein*, &c. the author cites M. Klipstein's late work on the Wetterau and Spessart; and he confirms the conclusions already drawn by Prof. Sedgwick in his comparison and identification of the same strata with the magnesian limestone of England.

New red sandstone series.—In this vast group the author, following the classification of Humboldt, Hoffmann, and other modern writers, points out that in Germany it is divided into three great systems; an inferior and a superior red sandstone, each abounding in variegated marls, the one separated from the other by that great limestone formation called the "*muschelkalk*". The lowest system or *bünter sandstein* being described first in general terms, detailed sections of it are then given from Alsace, where the author found it to be capped by *muschelkalk*, and charged with some peculiar plants, chiefly Coniferæ and Ferns, first discovered in it by M. Voltz, and since described by M. Adolphe Brongniart; he likewise found in it many bivalve and univalve shells, approaching very nearly in character to those of the *muschelkalk* and superior formations, but, as well as the plants, differing essentially from any fossils of the magnesian limestone and inferior formations. The frequent occurrence of salt and gypsum is noticed—numerous instances of great dislocations and elevations of the beds are enumerated, particularly on the northern flank of the Hartz—in the south of Hanover, a section across the Thuringerwald, by a new road, is given—and places are cited where the red sandstone is prismatized, in contact with trappean or igneous rocks.

Muschelkalk.—This most important limestone formation, averaging in thickness from 600 to 800 feet, is seen in Wirtemberg, Bavaria, Gotha, and Hanover, to rest upon the *bünter sandstein,* and to be capped

capped by *keuper*. A triple subdivision of the *muschelkalk*, established by Hausmann, is spoken of, in which each subdivision is characterized by its peculiar fossils.

For a full account of the *muschelkalk* of Wirtemberg the reader is referred to Alberti's *Konigsreich's Würtemberg*, by which it is shown, that all the salt-mines of that kingdom occur in this formation. The Saurian remains found in it by M. Jäger consist of Plesiosaurus, Ichthyosaurus, and an unknown reptile, in addition to which Count Münster has procured from the same limestone, the jaws and teeth of a crocodile, plates of a turtle, many parts of fishes of new genera, &c. By way of comparison with the *muschelkalk* of Germany, the author gives a sketch of the same formation in Lorraine, where the fine collection of M. Gaillardot of Luneville is specially spoken of, in which, in addition to many Saurian remains, there are bones of gigantic tortoises, with the characteristic fossils of the formation (such as *Ammonites nodosus*, *A. biplicatus*, *Mytilus socialis* (Schlot.), *Encrinites liliformis*), and two species of the remarkable fossil called Rhyncolites.

Keuper.—This formation of purple, red and green sandstone and marls is stated to be of enormous thickness at Stuttgart, where it is seen reposing on *muschelkalk*, surmounted by lias; and a detailed section at that place is given, in which are specified the beds of red sandstone containing the greatest number of the fossil plants described by M. Jäger. Calamites are mentioned as being found in the lower quarries, and in the upper certain Equisetaceous plants, which very much approach to the characters of the plants of the lias and oolitic series of England: 2 new species of Saurians, (Cylindricodon and Cubicodon of Jäger,) are also mentioned. The exact range of this formation in the North of Germany is to be found in Hoffmann's New Maps.

The author believes that the upper red and green marls of the English series are the true representatives of the *keuper*, and that the only group in the red sandstone series of Germany hitherto unobserved in England, is the *muschelkalk*; and he invites geologists to attempt to discover the equivalent (however feeble) of that limestone formation, by seeking for it as a bed of separation between the upper red marls and the lower new red sandstone of this island.

Lias.—The lias marls and gryphite limestone, with many identical species of English fossils, are stated to be well developed in Wirtemberg, the north of Bavaria, Hanover, Westphalia, &c.

After instituting a close comparison between the fossil contents of the lias of Wirtemberg and that of England, in Saurian and other animal remains, drawn chiefly from the work of M. Jäger, the author gives in great detail, a section on the right bank of the Maine at Banz, near Coburg, a spot to which his attention was first directed by M. de Buch, where the beds are very analogous in mineral characters and succession to those of the coast of Whitby, and where the most astonishing profusion of fossils has been collected through the industry of MM. Theodori and Gezer, all of which now ornament the Ducal museum of Banz. Amongst these are 6 species of Ichthyosaurus, 5 of which are known in England (*Ichthyosaurus tenuirostris* being

being the most abundant):—Fishes, 6 or 7 genera, (Dapedium, Clupæa, Cyprinus, &c.)—Pterodactylus—Crustacea, 2 species, Ammonites, 11 species, of which about two-thirds are figured in Sowerby's Mineral Conchology—Belemnites, 12 species, Scaphites, Nautili and numerous other univalves as well as bivalves common to the English lias. Some of the higher beds are described as containing Trochi, Helicinæ, and Spiriferæ. Pentacrinites Briareus of the English lias is likewise stated to be of common occurrence, and that a species of Fungia, a genus of corals hitherto unobserved in the lias of England, also occurs.

Inferior Oolite.—The inferior oolite of Germany is next described, as being quite analogous to that of the Hebrides and the coast of Yorkshire, viz., a great arenaceous formation, for the most part highly ferruginous. It contains many characteristic British fossils, and uniformly caps the lias throughout Wirtemberg, Bavaria, Hanover and Westphalia, and in some parts (near Banz and in Franconia) it passes up into an iron-shot, true oolite (*Oolitischer eisen-stein* of Münster).

The ferruginous grits of this formation, it is stated, are not to be confounded with the lias grits, from which they are clearly distinguished both by fossils and superposition.

A very detailed section is then given of all the strata exposed in the gorge, called the Porta Westphalica, by which the Weser escapes into the plains of Minden, and where all the sub-formations of the oolitic series, consisting of shales, grits, bands of oolite, &c. are well exposed. The beds are here considerably inclined, and include representatives of the English series, from the top of the lias to the shales of the age of the Oxford clay. All this system of the inferior and middle oolite, passes, it is observed, beneath the Bückeburg range of hills, containing sandstone and calcareous shale with workable seams of coal, which group the author agrees with M. Hoffmann in referring to the upper system of the oolitic series, and states that it contains many marine shells; whilst he distinctly shows that it is not the green-sand, of which there are clear sections in the immediate neighbourhood.

Middle Oolite.—Jura Kalk, &c.—The mineralogical characters of the middle oolite of central and southern Germany are pointed out as being essentially different from those of rocks of the same age in Westphalia and Hanover: so that instead of the shales, grits, &c. just described, they consist in one part of compact, cream-coloured limestone, and in another of dolomite. In Franconia (the great region of bears' caves), in the hills opposite Banz, and in many other places, the dolomite usually caps the limestone, the latter containing the greater number of the fossils. In these groups and in the inferior oolite, Count Münster has detected nearly all the species of Ammonites figured from this part of the series in the Mineral Conchology, with many other new species; and has also procured at least sixty species of Scyphia from the middle *Jura kalk*, and many other zoophytes now figured in Goldfüss.

Solenhofen Slate.—The Jura limestone or middle oolite is observed within

within a certain limited district, between Kehlheim on the S.E. and Pappenheim on the N.W., to pass upwards into a slaty, compact limestone, which is exposed in *plateaux* overlying dolomitic *Jura kalk* on both banks of the river Altmühl, but is of sufficiently fine texture, in only a few quarries near Solenhofen, to be worked as lithographic stone *. The quarries are then described, and their fossil contents, as collected by the author or observed by him in the collections of Count Münster and others, are enumerated. Seeing the prevalence of Pterodactyli, Insects, Crustaceæ, and Tellinites, and knowing that these fossils, together with certain plants, are also found in the Stonesfield slate of England, and further that these slaty beds at Solenhofen immediately surmount limestones, which by their contents are found to be the equivalents of the middle and inferior oolites of England on which the Stonesfield slate also rests—he is led to consider it probable that the Solenhofen and Stonesfield slates are of similar age; an opinion which he believes has been recently expressed by Dr. Boué.

The whole of this slaty group of Solenhofen, &c. is seen near the mouth of the Altmühl to thin out between masses of dolomite; the whole being surmounted by green-sand and cretaceous deposits.

The author inclines to the opinion that the higher members of the oolitic groups of England, viz. Coral Rag, Portland Stone, &c., have not yet been defined in any part of central Germany, though they may exist in Hanover; and he is unable to say whether the limestone of Nattheim, Heidenheim, &c., so abundant in corals, is referrible to the upper part of the great oolite or to the coral-rag.

Green Sand.—It is remarked that wherever this formation shows itself in Germany, it is nearly always divisible, as in England, into lower or siliceous sandstone, and upper or cretaceous sandstone; the former known in certain districts as the *quader sandstein*, the latter as the *pläner kalk*. Numerous sections exhibiting these two formations are given in various parts of southern Hanover and the northern flank of the Hartz, where the lower sandstone is sometimes an highly ferruginous rock, at others a white sandstone, in which character it ranges from the northern flank of the Hartz into Saxony and Bohemia. In Westphalia the green-sand series is said to approach still closer to the mineral type of the English group, and sections are described near Bidofeld, Soest Weil, &c. in which not only an upper and a lower green-sand with many characteristic fossils are described, but also traces of a separating stratum of blue marl or gault.

Chalk.—The author states that the chalk is quite as clearly separated from the *pläner kalk* in Hanover, as the chalk of the South Downs is from the malm rock or upper green-sand in Western Sussex. He remarks that on the northern flank of the Hartz, Professor Sedgwick and himself observed it to be quite vertical, whilst the underlying green-sands were by great faults thrown up into unconformable juxtaposition; and he further refers to a memoir recently read by himself, in which the chalk with flints is stated to occur in southern

* For a specific account of this range, see Von Buch's Letter to Brongniart, 1823.

Bavaria, resting in horizontal strata on the granite of the Bohemian mountains; and he points out as a necessary inference arising therefrom, that the Hartz and Böhmerwald-Gebirge have been elevated at distinct periods.

Tertiary Formations.—Those peculiar transition-tertiary formations described by Professor Sedgwick and the author at Gosau, and in the Austrian Alps, are stated to have been not as yet discovered in central Germany, but only along certain points encompassing it, such as at Maestricht, in the Baltic, the Carpathians, and the Alps. The true tertiary formations, though of considerable extent in different parts of the country, particularly in Hanover, Westphalia, &c., are stated to have been hitherto little attended to by native authors. Without endeavouring to give anything like a general account of the tertiary deposits of Germany, the author rapidly enumerates several localities where there are great exhibitions of sands, clays, lignite, &c. of the age of the plastic and London clays, particularly at Hesse Cassel, and the environs, where the brown coal, &c. of this epoch is traversed, and in parts prismatized by the overlying basalt (Meisner, &c.). The lower tertiaries are again spoken of as appearing in many points near Frankfort. In the environs of Mayence, Wisbaden, &c. it is shown that they pass upwards into a great estuary deposit of white limestone and marl, in which fluviatile and land-shells greatly predominate over those of marine origin, and at Monbach are associated with bones of large mammalia, so that the author inclines to the belief of the previous existence of a vast estuary or brackish lake in this spot, the waters of which have been let off by the fissure through the Taunus Mountains in which the Rhine now flows.

The low countries of Westphalia, Osnabruch, Bründe, &c. are specially cited as regions in which a vast development of tertiary marine strata exists; and little doubt is entertained that when fully examined they will afford representatives of most of the formations from the *calcaire grossier* to the crag inclusive, the latter having been already discovered at Antwerp, &c.

The deposits of unmixed lacustrine origin in central and southern Germany, such as Oettingen, Steinheim, &c. are merely named, having been already alluded to in a memoir upon Œningen, in which the author endeavoured to prove that deposit to be one of the most recent on the surface of the earth; and he terminates this communication with an account of a more newly discovered accumulation of the same nature at Georges Gemünd near Roth, which, from its organic remains, is proved to be of an age intermediate between the gypseous period of the Paris basin, and the youngest lacustrine formations. Beds of sandy marl, and whitish concretionary limestone are said to occur in isolated patches, crowning low hills of *keüper* sandstone at heights of about 150 feet above the present drainage of the district, and containing subordinate layers of calcareous, ferruginous and bony breccia, in portions of which, collected by the author, Mr. Pentland has discovered *Palæotherium magnum*; Anoplotherium, new species, resembling *A. commune*, and a new genus allied to Anthracotherium or Lopihodon. Mr. Clift has identified fragments of the teeth and

and bones of the hippopotamus, ox, bear, &c. Count Münster had previously collected from the same place, remains nearly similar, with the addition of *Palæotherium Orleani, Mastodon minutum, Rhinoceros pygmæus* (Münster), *Ursus spelæus,* and a small species of fox. Judging from the appearances on the spot and the evidences there offered of the gradual accumulation of this deposit, the author is of opinion that all these animals were of contemporaneous existence, and that this intermixture of quadrupeds of so old a period as the gypseous limestone of Paris, with others, the genera of which now inhabit our present continents, has supplied a valuable link in the chain of fossil zoological affinities.

The following books are referred to in the memoir: Keferstein, Teutschland, geognostisch-geologisch dargestelt; with Maps &c.—Boué, Synoptische Darstellung.—Boué, Geognostisches Gemälde von Deutschland, 1829.—Merian, Umgebungen von Basel, 1821.—Hoffmann, Nord-westlichen-Deutschland; with Maps, Sections, &c. Berlin, 1830.—Klipstein, Kupferschiefergebirge der Wetterau und des Spessarts, Darmstadt, 1830.—Alberti, Die gebirge des Königreich's Wurtemberg, 1828.—Schwatzenberg, Petrographische carte von Kreise, Cassel, 1825.—Von Buch, Letter to Brongniart, Journal de Physique, Oct. 1822.—Zincken, Ostliche Hartz, Brunswick, 1825.—Hausmann, Uebersicht der jungeren Flötzgebilde im Fluszgebiete der Weser, Göttingen, 1824.—Oeyenhausen, Von Dechen und De La Roche, Geognostische Umrisse der Rheinländer; with Maps, Sections, &c., Essen, 1825. Together with many memoirs in Leonhard's, Karsten's and other journals.

ASTRONOMICAL SOCIETY.

April 8.—The following communications were read:—

I. Extract of a letter, dated March 24, 1831, from M. Cauchoix, of Paris, to the Rev. R. Sheepshanks:—

"I have just finished an object-glass larger than that of Sir J. South. Two satisfactory trials of it were made on the 17th and 22nd of March, by Messrs. Bouvard, Arago, Mathieu, and Gambard, on the trapezium of *Orion, Venus,* a difficult double star, and *Saturn,* with magnifying powers of from 200 to 1000; and though the sky was covered with light whitish clouds, the images were distinct and brilliant, without any colour or scattered light. I subjoin the exact measures of the object-glass, and the various magnifying powers employed up to the present time. M. Gambard, who has made more observations with this telescope than any other person, thinks that stronger eye-glasses may be easily adapted to it.

	Old French feet.			Metres.	English feet.	
	f.	*i.*	*l.*		*f.*	*i.*
Diameter of the convex lens........	1	1	1·0	0·354	1	1·86
———————— concave flint-glass	1	0	7·0	0·341	1	1·50
———————— real aperture......	1	0	4·5	0·335	1	1·29
Focus of the object-glass............	23	7	5·0	7·700	25	3·00

Powers employed......201, 324, 397, 461, 786, and 1000.

 "These

"These powers are not exaggerated, but have been measured with scrupulous care; so that you may be assured no future trial will give them less than the numbers here quoted."

II. A communication from the Astronomer Royal, containing the results of all the observations, which have been made at the Royal Observatory at Greenwich, of the sun's zenith distance, at or near the solstices, from the time of Bradley to the present time.

Mr. Pond states the latitude of the Royal Observatory, which results from Bradley's observations, to be 51° 28′ 40″,323; and that which is obtained from his own observations to be 51° 28′ 38″,077; the difference being 2″,256.

III. A communication from Sir Thomas Brisbane, consisting of observations of the moon, and moon-culminating stars, during the years 1829 and 1830, made at Makerstoun with a four-feet transit. Sir Thomas Brisbane states in the letter accompanying his paper, that "the observations were almost entirely made by Mr. Dunlop." The paper contains the observed differences in Æ between the moon and the moon-culminating stars, observed before and after her culmination, and also the number of wires used in each observation.

IV. On the theory of the eye-glasses of telescopes; by Professor Littrow, Associate of the Society.

V. The reading of a paper by Francis Baily, Esq., "On Lacaille's Catalogue of 398 principal Stars," was begun.

May 13.—The following communications were read:—

I. Mr. Baily's paper, "On Lacaille's Catalogue of 398 principal Stars," was concluded.

In this paper Mr. Baily has entered into the merits of this celebrated catalogue. The method adopted by Lacaille for determining the right ascensions was that known by the name of *equal altitudes:* for when he commenced his astronomical career, the transit instrument was but little known, and not fully appreciated; and although it had been introduced into the observatory by Roëmer, yet it was soon abandoned for the mural quadrant, which at that time was considered a more manageable and accurate instrument, and capable of giving at one view both the right ascensions and the declinations. The instrument used for this purpose was a 3-feet quadrant: and he usually took about 14 or 16 altitudes on each side of the meridian. The culmination of the stars being thus determined, he compared them with *α Lyræ* or *Sirius* (whose absolute right ascension had been ascertained by frequent comparisons with the sun), and thus computed their right ascensions relatively to those stars. The declinations were determined with two different instruments: one a 6-feet sector, and the other a 6-feet sextant; the divisions of which were examined with great care by himself, and the trifling discrepancies noted and allowed for.

By the help of these instruments, Lacaille determined the positions of nearly 400 of the principal stars in the northern and southern hemisphere: and the catalogue which he thus produced, and which is the subject of the present memoir, is worthy of being placed in competition with those of his distinguished contemporaries. Mr. Baily has

has examined this catalogue with great care, and has discovered a few errors, which seem to have escaped the diligence of its illustrious author.

When Lacaille had finished his observations and his catalogue, he was at a loss for the means of publishing them; since his own limited income would not warrant the expense. He tried several modes of doing this, and at last agreed with his bookseller to compute for him an astronomical ephemeris for ten years, if he would print a sufficient number of copies for distribution amongst his scientific friends and correspondents. Such was the origin and cause of the *Fundamenta Astronomiæ*,—a work which was not published for sale, and which can therefore only be obtained accidentally from time to time as it occasionally passes from the hands of those to whom it was originally presented. It is consequently very rare; and those who possess it ought (as M. Delambre emphatically expresses it) to preserve it as a precious relic.

As the catalogue has never yet been reprinted, in the extended and perfect form in which it exists in that work, Mr. Baily conceived that he should be rendering an acceptable service to astronomers in presenting it to them, thus corrected, through the medium of this Society, accompanied with a comparison of the places of all the stars visible in this latitude, with those given by Bradley; and a reference to every observation of every star; together with notes accompanying the whole. Mr. Baily has also mentioned another motive which induced him to examine the comparative merits of this catalogue at the present time: since above one-third of the stars are so situated that they cannot be observed in this latitude, on account of their great southern declination. These stars, therefore, he justly remarks, will afford a favourable opportunity of comparison with the same stars as observed by Sir Thomas Brisbane at his observatory at Paramatta; whose valuable and extensive observations are now in the course of reduction at the public expense; and thus enable us to determine, with considerable precision, the annual variation of at least 136 principal stars in the southern hemisphere. Mr. Baily closes his paper with a few remarks on the scientific life of Lacaille.

II. A letter from Mr. Dawes, dated April 23, 1831, of which the following are extracts:—

"I beg to call the attention of the Astronomical Society to an interesting circumstance respecting the triple star ζ Cancri. I am not aware of any published observations relative to the positions of the stars composing it, since those of Sir James South, made at Passy in 1825. Possibly, however, such may exist, and may render my present communication unnecessary, or, at any rate, less interesting.

"The evenings of the 19th and 20th of this month proving favourable for delicate observations, I directed a five-feet achromatic telescope to this object. This instrument was constructed for me last year by Mr. Dollond; and from the perfectly round, clean, and very small discs with which it exhibits the fixed stars, it is peculiarly adapted for the examination of very close and delicate objects. It is mounted equatorially, with horary and declination circles, each of two feet

feet diameter, and is furnished with a position and parallel-thread micrometer, possessing a variety of magnifying powers, from 55 to 625. The instrument was fitted up with a special view to the examination of the positions and distances of the multiple stars. Each part of the micrometer screw-heads is equal to 0″,5559219."

Mean result. Pos. of AB = 59° 13′ *nf* (16 obs.) Dist. = 1″,095 (7 obs.)
Pos. of AC = 60 17 *sf* (5 obs.) Dist. = 5 ,592 (5 obs.)

After giving the observations, Mr. Dawes proceeds thus:—

"Now, if we compare these results with the measurements of Sir W. Herschel in 1781, and of Sir James South in 1825, a very extraordinary variation appears. In 1781·90, Sir W. Herschel's measure of the position of AB was 86° 32′ *nf*. In 1825·27, a mean of 43 measures (the means of the *six* different sets on as many nights, agreeing admirably together) gave as the position 32° 10′ *nf*, offering a difference of 54° 22′ from the position in 1781. *Now*, however, the angle lies as nearly as possible midway between the two, being 27° 19′ less than that of 1781, and 27° 3′ greater than that of 1825; while the distance, as measured by me, is only 0″,009 greater than the result of 15 observations by Sir J. South. It would therefore, appear as if the motion had been performed in a *direct* sense (or *nf sp*) for perhaps thirty or forty years; and that the star B had then come to a stand, (or *appeared* to do so), faced about, and is now proceeding in the *opposite* direction, which is the same pursued by the star C. But this is an extravagant idea; and unless there exist observations in the intervening period to invalidate the supposition, I think we may arrive at a solution of the difficulty in a more simple manner; namely, that in the forty-nine years elapsed since Sir W. Herschel's measurement, the star B has performed *almost an entire revolution*, in a retrograde sense, or *np sf*, only about 27° or 28° being wanting to complete it.

"Mr. Herschel, in his notes appended to Sir J. South's valuable paper, forming the first part of the *Philosophical Transactions* for 1826, has mentioned an observation of this star by his father in 1802, 'at which time,' it is said, 'no measures could be procured.' Now, supposing this to be the only observation recorded during the interval, it may be asked, Would not Sir W. Herschel have been struck with any remarkable alteration in the relative positions of A and B, even supposing the circumstances were not such as to admit of measurements? To this it may be replied, that it is highly probable the star B would, about the year 1802, have arrived at that point of its orbit in which it would be nearly *opposite* to its place in 1781; and the stars differing but little in size, it might not be noticed, especially in unfavourable circumstances, which *preceded*; and, consequently, it might easily be imagined to have sustained but little, if any, alteration.

"The position of B now observed is the more remarkable, because, had it pursued the course formerly assigned, its angle relative to A would now be about 24° or 25° *nf*. That an error of 35°, or any thing like it, should be committed in my measurements, appears quite improbable; since, if we examine them, we shall find that the extreme difference of all the sixteen measures taken on the two evenings is only

only 3° 35′. And I may here observe, that in measuring both positions and distances, I always *derange* the former measurement so completely, that the situation of the wire could not by possibility be taken for a measure. Thus, each individual measure is not a previous one, altered and amended, but is entirely new and independent.

"It will be seen that the above observations corroborate the presumed motion of the star C in *direction*, and indicate a considerable acceleration in it since 1825, compared with the *mean* velocity during the previous forty-two years; while the distance continues very nearly the same as it was at that date. Compared with Sir J. South's observations, the change of angle amounts to 7° 38′ in six years, or 1°,267 per annum; while, previously, the mean motion was supposed to be only 0°,5813. But in the observations appended to the measures by Messrs. Herschel and South, in 1822, we were forewarned to expect an acceleration, *if the motion were orbitual.* My observations of the star C, however, being made on one night only, may be liable to an error of one or two degrees, especially as the position of the star B, and its extreme proximity to A, render the measures of C difficult."

III. An Index to the Society's Catalogue, by Lieut. W. S. Stratford, R.N.; so arranged that the number of any star contained in the Catalogue may be found immediately, provided there be given the name of the constellation, and the letter by which the star is distinguished, or the number of the star in one of the following catalogues; viz. Flamsteed's, Piazzi's, Bradley's, Lacaille's, Fallows's, Zach's, &c., preference being given to each catalogue in the order of succession.

IV. A paper containing micrometrical measurements of 364 double stars, by J. F. W. Herschel, Esq.

ZOOLOGICAL SOCIETY.

April 26, 1831. Joshua Brookes, Esq. in the Chair.

Mr. Vigors exhibited, from the collection of Mr. Leadbeater, an undescribed species of *Cockatoo* from New Holland, and pointed out its distinctive characters, which may be expressed as follows:

Plyctolophus Leadbeateri. *Plyct. albus; genis, collo in fronte, pectore, tectricibus alarum inferioribus, abdomineque medio roseo-tinctis; cristæ elongatæ occipitalis plumis basi roseis, apice albis, maculâ flavâ in medio notatis; pogoniis remigum rectricumque internis roseis, illorum saturatioribus.*

Statura *Plyct. sulphurei*, Vieill.

Eleven species of *Chætodons*, forming part of the collection of *Fishes* from the Mauritius presented by Mr. Telfair, were laid on the table. Seven of these were referable to the genus *Chætodon* as restricted by M. Cuvier; and among them Mr. Bennett pointed out more particularly the *Chæt. strigangulus*, Sol.; the *Chæt. vittatus*, Schn.; the *Chæt. Lunula*, Cuv. & Val.; and two species which he believed to be new to science, and which may be thus characterized:

Chæt. flavescens. *Chæt. flavus; ore, fasciâ oculari, lineâ pinnas dorsalem analemque posticè ambiente, apiceque pinnarum ventralium*

ventralium nigris; lateribus argenteo vittatim guttulatis; pinnâ caudali rectâ, apice latè hyalino.

D. $\frac{13}{22}$. A. $\frac{3}{18}$, &c.

Affinis, ut videtur, *Chæt. virescenti*, Cuv. & Val. Differt colore flavo; pinnis verticalibus posticè nigro tenuiter cinctis; lateribus obscurè argenteo-guttulatis.

CHÆT. ZOSTER. *Chæt. brunneo-niger; zonâ latâ mediâ ventreque argenteis; pinnâ caudali rectâ albâ: fasciâ oculari nullâ.*

D. $\frac{12}{25}$. A. $\frac{3}{21}$. P. 17. C. 15. V. $\frac{1}{5}$.

The remaining species exhibited types of the genera *Heniochus*, Cuv.; *Zanclus*, Cuv. & Val.; *Holacanthus*, Lacép.; and *Platax*, Cuv.: the *Heniochus* being the species recently described by MM. Cuvier and Valenciennes as the *Hen. monoceros*. In this individual the spine in front of each orbit is strong, almost equalling the single spine which projects from the middle of the slope of the head; and the whole contour of the anterior part of the fish approaches very nearly to that of *Taurichthys*, Cuv. & Val.

Mr. Gray exhibited several living specimens of the *Rana Rubeta*, L., the *Natter-jack* of Pennant, a reptile intermediate in form and habits among the British *Amphibia* between the *Toad* and the *Frog*. He stated that this animal, the indigenous existence of which has frequently been doubted, is found abundantly on Blackheath, and on other commons in the neighbourhood of London.

Mr. Gray also exhibited several specimens of the genus *Rhynchæa*, Cuv., and pointed out from among them two distinct species, which may be thus characterized:

RHYNCHÆA CAPENSIS, Sav. *Rhynch. remigibus angustis, fasciis latis flavis sex notatis, infrà griseis, nigro-vermiculatis, flavoque fasciatis; secundariarum maculâ pogonii externi, fasciâque pogonii interni, flavis.*

Long. corporis $9\frac{3}{4}$ unc.: tarsi, $21\frac{1}{2}$ lin.: digiti unguisque medii, $20\frac{1}{2}$ lin.

RHYNCHÆA PICTA. *Rhynch. remigibus sublatis, externis flavo latè 7-fasciatis, infrà griseo nigroque vermiculatis, interno obsoletè flavo-fasciato: secundariarum apicibus, maculâ ultimâ fasciæformi pogonii externi, fasciâque pogonii interni, albis.*

Long. corporis $10\frac{1}{2}$ unc.: tarsi, $19\frac{1}{2}$ lin.: digiti medii, 19 lin.

The wing-coverts of both species are spotted with yellow in the young state; and in the adult state are metallic olive with black bands.

Mr. Gray added that the three figures of birds of this genus which were published by Buffon, and which had of late years been regarded by M. Temminck and by M. Cuvier as representing various states of but one species, were none of them sufficiently correct in the details to enable him to refer either of the present species to the representations given in the 'Planches Enluminées;' but that the figure of the *Rhynchæa Capensis* given by Savigny in the 'Oiseaux d'Egypte' [tab. 14. fig. 2.], furnished a faithful representation of the first species exhibited by him. He had not, however, obtained this bird from the Cape of Good Hope, his specimens being from India

India and China. The second species, *Rhynchæa picta*, he had received from Africa as well as from India and China.

Mr. Vigors called the attention of the Committee to the *Frigate-bird* (*Tachypetes Aquilus*, Vieill.), and dwelt upon those peculiarities of its organization which point out its station in the series of natural affinities that connect the orders of birds. Although it possesses the webbed feet which constitute the technical character of the Natatorial Order, the weakness of its legs and their complete covering of feathers preclude it from employing these members in the same manner as the typical groups of the Swimming Birds; while on the other hand its great powers of wing and tail adapt it for powerful and long-continued flight, and evidently connect it with the Raptorial Order, which it also resembles in its manner of taking its food. It is in fact rather an inhabitant of the air than of the water: and it has been believed that it derives support during its unlimited flights not merely from the strength and expansion of its wings and the singular mechanism of its tail, but also from the buoyant nature of the inflated sac beneath its throat. A proof of the correctness of the opinion that this pouch is really an air-sac, and that it is filled with air, which passing through the bones becomes rarified and capable of imparting a high degree of buoyancy, has recently been obtained from the anatomical notes made by Mr. Collie, late Surgeon of H.M.S. Blossom, who accompanied Captain Beechey in his voyage to Behring's Straits; notes which will shortly be published in illustration of the natural history of that expedition. "The pouch beneath the throat of this bird," says Mr. Collie, "is of a yellowish red colour, and when distended, the feathers on its upper and posterior surface are separated to some distance from each other, and exhibit very distinctly the quincuncial order in which they are implanted. On first looking at this pouch, I was a little surprised at finding that it did not communicate with the mouth or *fauces* in any way that I could perceive. I succeeded in inflating it only by long and forcibly blowing into the *trachea*. I desired the man who had the skinning of the specimens brought on board to inflate the pouch before commencing the skinning, and to let me know when he had advanced to the shoulders. He however dislocated the shoulder-joint first, when the distended pouch immediately collapsed. The *trachea* had been tied. As soon as I was informed of this, I had little doubt that the pouch had been inflated from the lungs; and on observing two wide openings, one anterior to the humeral articulating face of the *scapula*, the other the usual opening of the joint, I hesitated not to infer that it was through the first of these the air had passed in, and that the dislocating of the joint, by which its capsular ligament was torn, had allowed the air to escape at the opening which corresponds to that on the head of the *humerus*, and which immediately leads, as well as the other just mentioned, into the centre of the *scapula*. I now opened the *trachea* immediately before the *sternum*, and again attempted inflation from that part, but in vain. I tried it also, but with no better success, from the *larynx*. I next examined with the

blowpipe near the opening of the *scapula*, in the cellular substance under the skin, and soon detected a small opening that conducted the air to the pouch, which was readily inflated by blowing through the opening, and so long as it was shut the pouch continued distended. That this opening was not artificial,—the effect of the rupture of the fine membrane lining the air-bladder,—was evident from its not opening directly into it, but only after a passage of some length, gradually enlarging. That this was the sole opening into the pouch appears proved from the fact that after detaching the sac from all the parts beneath, *i. e.* from all the parts excepting the skin, it did not permit the gas to escape except by this opening, and that it continued to be capable of inflation from it. I was satisfied in discovering it on one side; and of course inferred that it was similar on the other, the opening of the *scapula* being similar."

At the request of the Chairman, Mr. Martin read the following notes of the dissection of a female *Testudo Græca*, Linn., which died in the possession of Oct. Morgan, Esq. The animal was of the usual size, its dimensions being as follows: the *carapace* in length 13 inches; the *plastron* $9\frac{1}{2}$ inches in length; and the circumference of the shell, 18 inches.

"The *plastron* being removed, the *viscus* which first attracted notice was the liver, of large dimensions, stretching across from side to side, and quite covering the stomach. Its structure was very firm, and its colour a dull ochre. It consisted of two lobes, both deeply fissured. In the cleft of the right lobe was situated the gall-bladder, of the size of a large nut, and containing green bile. The cystic and hepatic ducts united, and entered the *duodenum* $1\frac{1}{2}$ inch below the *pylorus*.

"On the liver being turned aside, the stomach presented itself; its coats were firm and thick, especially in the pyloric portion, which was produced long and narrow to the extent of $3\frac{1}{2}$ inches; the total length of the stomach was $6\frac{1}{2}$ inches.

"The small intestines, remarkable also for their firmness, measured 2 feet 8 inches in length, and terminated in large intestines very little exceeding them in circumference. In the *Testudo Indica* lately dissected, there was no *cæcum;* but in the present species the *cæcum* existed; its form was globular. On the left side the large intestine assumed a sigmoid flexure with a bold sweeping fold, and then took on a straight and short course to the *cloaca;* the length of the large intestines was 1 foot 8 inches. They contained fæculent matter in small quantity, consisting of fibrous vegetable substance. There were no longitudinal bands.

"The *cloaca*, into which opened the bladder and oviducts, was in length 2 or 3 inches. The bladder in the present instance did not exhibit that immense volume which was so remarkable in the *Test. Indica:* it was of a moderate size; both in this respect and in figure resembling a pear. It was united to the sides of the upper shell by a broad peritoneal ligament, and was connected also to the *pelvis* by several fibrous bands. Its coats were extremely thin and fibrous; and it contained a small quantity of thick fluid.

"The

"The oviducts were before their opening into the *cloaca* united for a considerable distance, and were there thick and firm, becoming gradually thinner as they proceeded upwards, their course being in an indefinite convoluted manner. Throughout the greatest part of their length there ran a number of longitudinal folds, which became fainter, and were at length obliterated as the oviducts proceeded.

"The ovaries contained a multitude of eggs of various sizes, and of a round figure; fifty of them at least were nearly as large as a pigeon's egg: they were not covered with a shell, and were filled with a thick yellow yelk.

"The kidneys laid upon the lungs (which extended over the *carapace*), to which they adhered; their figure was somewhat 3-sided, from a broad flat base, with a rounded *apex:* their length was 2½ inches. Their surface was convoluted in a very singular manner, the folds being divisible, producing an appearance not unlike that of the *cerebellum*, which they also resembled in colour.

"On the *mesocolon* and near the intestine was situated an oval glandular body of a dark colour, and of the size of a sparrow's egg, containing white gritty specks. From this, which I suspected to be the spleen, a large vein proceeded along the mesentery, and uniting with several others, entered the liver; all the veins proceeding from the *viscera* along the mesentery were very large and full of dark blood.

"The tongue was thick and fleshy, about an inch in length and two-thirds in breadth, white in colour, and covered thickly with elongated *papillæ;* the tip was rounded, the base heart-shaped. Between the *glottis* and base of the tongue so slight a distance intervened, that the *larynx* might be said to open directly into the mouth, the *glottis* rising to a point corresponding with and adjusted to the heart-shaped indentation at the base of the tongue. This elevated *apex* is divided downwards and a little way longitudinally by the *rima*. The *larynx* is supported posteriorly by the *os hyoides*, which is broad, flat, and pointed with double barbs, resembling some double-barbed arrow-heads: it is however composed of three bones, viz. a body, and two long curved bones united by cartilages to it, the body itself ending in two long cartilaginous processes; where the osseous processes arise there is also on each side a small cartilaginous projection. An inch below the *rima* the *trachea* divides into two branches, or *bronchiæ*, which run down for a little way on each side of the neck, but shortly, in consequence of the bend of the neck, almost at the back of it, and describing in their course a large sigmoid inflexion, they then subdivide and immediately enter the lungs. About half an inch below the great division a strong muscle of two or three lines in breadth passes across, arising from the *vertebræ* of the neck on one side and united to the same on the opposite, thus acting as a *constrictor* on the two tubes, and being doubtless of use in the deglutition of air. The length of the *trachea* and the great branches to the lungs was 7½ inches; the rings were perfect. The subdivisions of the *bronchiæ* before entering the lungs are surrounded closely by numerous yellow glands."

May 10, 1831. W. Yarrell, Esq. in the Chair.

A letter, addressed by Richard Thursfield, Esq. to Dr. Roots, was read, in illustration of the history of a hybrid between the *Hare* and the *Rabbit*, which was lately living at the Society's Farm. A gentleman who was rearing a pair of tame rabbits, placed with them, when they were about two months old, a young buck hare apparently about the same age, which became in a short time as domesticated as its companions. When the doe rabbit was old enough, she had, by the buck rabbit and the hare, a litter, consisting of three young ones, which resembled in all respects the mother and buck rabbit, and of three mules. Two of these mules shortly died: the third, a female, was reared with rabbits of her own age, and when six months old produced one young one: she was afterwards bred from eight times, by tame rabbits and by a wild one, but no opportunity occurred of placing a buck hare in confinement with her. Her progeny by a white tame rabbit, with which she bred twice, consisted of two young ones, which were perfectly gray, and of two which were spotted: the latter are still alive, and breed regularly, producing from five to eight at a time. The average weight of the progeny of the mule female was about five pounds; one, however, weighed six pounds and a half. She died shortly after coming into the Society's possession.

Mr. Owen, having examined the body of this hybrid animal after its death, reported that its size and colour were those of the *Hare*, but that its hinder legs were shorter than in that species, and agreed rather with those of the *Rabbit*. The length of its small intestines corresponded with that of the hare; its *cæcum* was seven inches shorter; while its large intestines measured one foot more than those of the hare.

Mr. Bennett called the attention of the Committee to the specimen of the *Sociable Vulture* (*Vultur auricularis*, Daud.), which has been an inhabitant of the Society's Gardens for nearly two years. His object in adverting to this bird was to correct an erroneous impression which might be produced on the minds of those who had never seen an individual of the species, by the statement made by M. Ruppel, in a late Monograph of the genus to which it belongs, that considerable doubts as to the existence of such a species might reasonably be entertained. M. Ruppel's doubts appear to have been excited by the fact which he reports, that the stuffed skin in the collection of the Duc de Rivoli at Paris, which has been regarded as that of the *Vult. auricularis*, is evidently factitious; the folds of the skin on the head and neck having been produced in that specimen by artificial means. These doubts must, however, be at once dissipated by the existence of a living specimen brought from the Cape of Good Hope, according in every particular with Le Vaillant's description of the *Oricou*, and having the remarkable folds of skin which pass up the sides of the neck and round the ears developed even to a greater extent than is represented in his figure. A specimen of the *Pondichery Vulture* (*Vultur Ponticerianus*, Daud.), the only other

other species in which the naked neck has on each side a longitudinal fold of skin, was laid on the table: and it was pointed out that in this bird the fold of skin terminates an inch below the opening of the ear, while in the *Sociable Vulture* it passes upwards and surrounds the upper part of the ear; and that the breast-feathers of the *Pondichery Vulture* are short and rounded, while those of the *Sociable Vulture* are very long and somewhat sabre-shaped.

Mr. Gray stated, that since M. Ruppel's Monograph was written, he had apprised that scientific traveller, in answer to his previous inquiries on the subject, that a specimen of another vulture rejected by him as a doubtful species (the *Vultur Angolensis*, Lath.) exists in the British Museum, to which it was presented on the return of the unfortunate expedition up the river Congo.

Mr. Owen resumed the reading of his Memoir on the Anatomy of the *Orang Utan* (*Simia Satyrus*, L.), portions of which had been communicated by him to the Committee at several of its previous Meetings. On this occasion he limited himself to the myology of the lower extremities.

He commenced by remarking, that no anatomist can contemplate the lower extremity of a Quadrumanous animal, or experience the degree of mobility of which the several parts of it are susceptible in the living or undissected body, without being prepared to find corresponding modifications of the muscular system and consequent deviations from the structure of these parts as they exist in man. It is accordingly in this part of the body that the most remarkable differences in the forms, proportions, and attachments of the muscles are found to obtain between the ape and the human subject; and it will not therefore be matter of surprise to find, that in the *Orang Utan*, whose inferior extremities, from their shortness and flexibility, are so well adapted to the various agile movements of a climber, there exists a high degree of this deviation from the human structure, and an approximation, in some measure symmetrical, to the arrangement of the moving powers in the upper extremity. Variations of more or less consequence occur, indeed, so frequently as to render it necessary to consider the whole of the muscles *seriatim;* and each of them was accordingly described separately as regarded its attachments, form, and relative position. These details are necessarily abridged in the present abstract, except as regards the muscles of the hinder hands, which require a developed notice to render their structure intelligible.

The *glutæus magnus* is a thin narrow muscle, inserted lower down the thigh bone, and having a more posterior origin than in man: its extent of action is consequently increased, though its strength is diminished. The *glutæus medius* is also relatively longer than in man, and is four times as thick as the preceding muscle. The *glutæus minor* is narrow, long, and thin. The *pyriformis* is narrower than in man. The tendon of the *obturator internus* passes as usual between the *gemini*, of which the inferior is much the largest. The *obturator externus* is considerably larger than the *internus*. The *quadratus femoris* has very little of the square in its shape, being much

much longer than it is broad, and becoming narrow and rounded at its insertion.

The *biceps cruris* consists of two portions, each maintaining a distinct course and having a distinct insertion: one of these may be termed *ischio-fibularis*, and is inserted into the head of the *fibula;* the other may be termed *femoro-fibularis;* its insertion is into the outer edge of the *fibula* from the head to the middle of the bone, and into the *fascia* in front of the leg. The *semitendinosus* and *semimembranosus* have the same origins as in the human subject, and the latter muscle a similar insertion; but the *semitendinosus* separates from it at the lower part of the thigh, and continues fleshy for some distance below the knee-joint; after which the tendon expands into a broad strong *aponeurosis*, which is attached along the anterior and inner aspect of the *tibia* to within a short distance of its lower extremity. In its insertion, the *semitendinosus* of the *Chimpanzee* approaches more nearly to the human type, being implanted by a narrower tendon in front of the *tibia* immediately beneath the insertion of the *gracilis;* but both these muscles are inserted lower down than in man.

Mr. Owen remarked, that the names of these last-mentioned muscles by no means agree with the proportion of tendon found in them either in the *Orang* or the *Chimpanzee*, the fleshy portion being in these animals of much greater extent;—a fact which is in accordance with a law that receives many illustrations from the myology of the *Orang Utan*, viz. that the extent of the fleshy part of a muscle is in proportion to the quantity of motion it has to produce: and this is generally indicated by the degree of motion allowed by the structure of the joint which is the centre of the motion in question. Thus in the human subject it is very rare that an individual can, by the contraction of the flexors of the leg, bring the heel in contact with the back of the thigh; but in the *Orang Utan* this action is readily performed, and without the slightest opposition at the knee-joint.

The *tensor vaginæ femoris* exists distinctly in the *Chimpanzee*, but no trace of it was found in the *Orang*. A more powerful *rotator* of the thigh inwards exists in both animals in a peculiar muscle, which may be termed *invertor femoris*. It was first discovered by Dr. Traill in the *Chimpanzee;* and its origin, form, and insertion in that animal agree with those which are met with in the *Orang Utan*. Mr. Owen considers that from its insertion into the under and outer part of the *trochanter major*, and consequently very near to the centre of motion, it can have little effect in drawing the thigh up towards the body as compared with the power of the proper flexors of the thigh. It appears rather to have reference to that structure of the hip-joint which, in the *Orang* especially, from the absence of the *ligamentum teres*, and in the *Chimpanzee*, from the yielding texture of that ligament, permits a greater extent of inward rotation than can be accomplished in man.

The *sartorius* is inserted lower down than in man. The *rectus cruris* corresponds with the same muscle in the human subject; but the

the *vasti* and *cruræus* are much weaker and thinner, and are evidently little adapted to support the thigh and trunk upon the *tibia*.

The *psoas magnus* and *iliacus internus* are, on account of the form of the *pelvis*, proportionally longer muscles than in man. Beneath them exists a small distinct muscle passing from the fore part of the *ilium*, over and attached to the capsule of the hip-joint, to be inserted into the root of the *trochanter minor*. This muscle is not found in the *Chimpanzee*. The *pectineus* is a narrower muscle than in man, and gives off, in the *Chimpanzee*, a small slip, which is continued under the femoral vessels and outwards to the origin of the *sartorius*. The *gracilis* is a very powerful muscle in the *Orang*, but is comparatively of less bulk in the *Chimpanzee*, in which it is inserted beneath the *sartorius*. On this muscle being removed, a number of others appear passing from the *pelvis* to the inner part of the thigh, among which it is difficult to select those which are precisely analogous to the muscles in the corresponding region of the human subject. Mr. Owen, however, distinguished the *adductor longus*; an accessory *adductor* arising from the upper part of the *symphysis pubis*; the *adductor brevis*; and the *adductor magnus*.

The *gastrocnemius* preserves nearly a uniform thickness and breadth throughout its course, and is continued fleshy down to the *os calcis*: it has no sesamoid bone, as possessed by some monkeys (e. g. *Macacus cynomolgus*, Lacép.), at either of its origins. The *soleus* has only one origin, and is continued fleshy to the *os calcis*. The tendon of the *popliteus* contains, behind the knee-joint, a fibro-cartilaginous sesamoid body, which was noticed by Camper, who states that it exists also in baboons, dogs, cats, &c.: this body, however, is not found in the *Chimpanzee*.

In the *Orang Utan* there are some important differences in the disposition of the flexors of the toes, as compared with the *Chimpanzee* and inferior *Simiæ*; thus the muscle analogous to the *flexor longus pollicis pedis* sends no tendon whatever to the thumb of the foot, and its origin is extended above the knee-joint in a manner analogous to the *flexor sublimis* in the upper extremity. It has two origins, one from the outer condyle in common with the *gastrocnemius internus*, the other from the head of the *fibula*, and is continued down the posterior part of that bone and the interosseous ligament to within an inch of the *tarsus*; under which it passes through a broad synovial sheath, deeper seated than, and external to, the *flexor longus digitorum*; becoming tendinous centrad, but continuing fleshy on the dermal aspect till it has reached the sole. There it divides into two stout perforating tendons, which are inserted into the distal *phalanges* of the third and fourth toes. Immediately after the division each tendon gives origin to a *lumbricalis* muscle, which terminates in a thin *aponeurosis* attached along the tibial side of the proximal *phalanges* of the third and fourth toes.

The *flexor longus digitorum pedis* arises as in the human subject, but continues fleshy till it has passed under the *abductor pollicis*; it then gives origin to a *lumbricalis* muscle, and divides into three tendons. The *lumbricalis* terminates in the middle tendon of the three.

three. The innermost or first tendon goes to the distal phalanx of the second toe; it also gives rise to a *lumbricalis*, which is inserted into the tibial side of the proximal *phalanx* of the same toe. The second tendon, after receiving the insertion of the *lumbricalis* before mentioned, goes to form the perforated tendon of the fourth toe. The third or outer tendon is inserted into the distal *phalanx* of the fifth toe, and also gives origin to a *lumbricalis*, which terminates in the tibial side of the proximal *phalanx* of the same toe.

The *flexor brevis digitorum pedis* arises from the posterior part of the *os calcis*, its fibres passing transversely over the insertion of the *tendo Achillis*. At about two inches from its origin it gives off a small tendon, which is inserted into the second *phalanx* of the second toe. It then continues fleshy for an inch further, and terminates in the perforated tendon of the third toe.

Thus all the toes from the second outwards, have a *flexor* tendon inserted into the distal phalanx: they have also a *lumbricalis* tendon attached to the proximal *phalanx*, and the second, third, and fourth have tendons inserted into the middle *phalanx*. As each perforating tendon gives origin to the *lumbricalis* muscle of its respective finger, these not only assist in the flexion, but act as guys on the tendons, from which they originate, preventing them from starting from the long concavity of the sole over which they travel: they also afford a variety of independent motions to the fingers. The *tibialis posticus* has the usual origin; its tendon passes along a distinct sheath close by the internal *malleolus;* it is inserted into the *os cuneiforme internum*. The tendon has no sesamoid bone where it passes over the *astragalus*. In the *Chimpanzee* it is inserted into the *os naviculare*.

The muscles in front of the leg are covered with a strong *fascia*, into which the tendons of the *semitendinosus* and *biceps* are inserted; it affords origins for the muscles situated beneath it, and becomes very strong at the ankle, binding down and forming sheaths for the several tendons. The *tibialis anticus* arises from the anterior inner and posterior aspects of the *tibia*, embracing it, as it were, and giving the appearance of a rickety convexity to the leg; it passes over the *malleolus internus* posterior to the centre of motion, and is consequently an *extensor* of the foot; it also turns the sole inwards. In close connection with this arises another muscle, not found in man; it becomes tendinous about three-fourths down the leg, and is inserted into the base of the metatarsal bone of the thumb, which it extends: this muscle is found in the *Chimpanzee*, and also, according to M. Cuvier, in the inferior *Simiæ*. The *extensor longus pollicis* makes its appearance as usual between the *tibialis anticus* and *extensor longus digitorum;* it is inserted into the base of the *phalanx:* (the female specimen that was dissected had only one *phalanx* to the hinder thumb). The *digitorum tensor longus* has the usual origin, continues fleshy to the ankle-joint, there divides into three tendons, which diverge at the middle of the foot, and are attached to the third, fourth and fifth toes; each tendon expanding into a sheath over the back part of the *phalanges*.

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The *extensor brevis digitorum pedis* arises from the *os calcis*, and divides into three portions; the strongest of which gives two tendons to the second toe, one being inserted at the base of the proximal *phalanx*, the other expanding over the second and distal *phalanges* like the tendons of the *extensor longus*. The remaining portions go to the fibular aspect of the third and fourth toes.

The *peroneus longus* and *brevis* arise together from the outer, fore, and back part of the *fibula*; on the latter aspect they are in connection with the *flexor longus pollicis*. The tendon of the *peroneus brevis* is inserted into the base of the metatarsal bone of the little toe. The tendon of the *peroneus longus* passes under the cuboid bone, without the interposition of a sesamoid bone, crosses the foot, and is implanted into the metatarsal bone of the thumb of the hinder hand, of which, as far as the structure of the articulation will permit, it is a *flexor*. There is no *peroneus tertius*.

The thumb is very short, consisting, in the female at least, of only two bones, set on at right angles to the foot, and at a great distance from the toes. In this part, however, the power of a considerable muscular apparatus is concentrated. Receiving no tendon from the *flexor longus pollicis*, it is rendered more independent in its actions; not being necessarily flexed, except in the action which turns down that side of the foot to which it is attached, and by which it is brought closer to the object to be seized. On the sole of the foot we find an *abductor* and an *adductor pollicis*, both powerful muscles inserted at very open angles into the *phalanx*; which, when they cooperate in their contraction, they must draw down in the diagonal with great force. Between these are situated two more direct *flexors*, constituting what is usually termed the *flexor brevis pollicis*.

The space between these muscles, which in man and the *Chimpanzee* is filled by the tendon of the *flexor longus pollicis*, in the *Orang Utan* is occupied by a small peculiar muscle which arises from the metatarsal bone, and is inserted into the *phalanx*. In a young male *Orang* that had two *phalanges* the *flexor brevis* was inserted partly into the second *phalanx*. The *extensor brevis pollicis* arises from the *os naviculare* and *os calcis*, and is inserted into the base of the proximal *phalanx*, when there are two.

On the *dorsum* of the foot may also be observed *interossei externi* of a penniform shape; they are attached to the fibular aspect of the proximal *phalanges* of the toes. There was also an *adductor minimi digiti*, and *interossei interni*, but not any trace of *transversalis pedis*.

Mr. Owen concluded his observations with some remarks on the structure of the principal joints of the lower extremity, and on the degrees of mobility of which they are susceptible.

In the hip-joint the most remarkable circumstance is the freedom of motion in the rotation inwards; this is, however, more limited than in the opposite direction. The motions of flexion and extension, abduction and adduction, are also very free. On examining the cause of the limitation of the inward rotation, he found it to be

a strong band of ligamentous fibres arising from the posterior margin of the cotyloid cavity, and passing along the back part of the capsule to the root of the great *trochanter;* when this was divided the rotation inwards was as free and extensive as happens in other cases after a division of the *ligamentum teres.* The synovial membrane is reflected over a greater part of the anterior and upper than of the back and under part of the *cervix femoris.* The marginal ligament of the articular cavity is four lines in depth, a remarkable thickness for the size of the cavity. The blood-vessels enter the joint by the usual notch, and supply abundantly the process of synovial and adipose substance called the gland of Havers.

The motion at the knee-joint is sufficiently free to allow the heel to be brought to the buttock, and even beyond, as in natural flexion it is carried external to the thigh. The only circumstances remarkable in the structure of the joint are, that the internal lateral ligament is longer, and the *ligamentum mucosum* stronger and of a more ligamentous nature, than in the human subject.

The motion at the ankle-joint is so free, that the *dorsum* of the foot can be brought into apposition with the fore-part of the leg; and it is worthy of remark, that when this motion is produced, the effect on the tendons passing behind the ankle-joint is such, as to cause a flexion of the toes similar to that which is produced in perching birds by bending the *tarsus* upon the leg. In the opposite direction the foot may be brought so far back as to form a right angle with the leg. Lateral motion is also very free, especially the turning of the sole inwards, to which aspect it naturally inclines. A certain degree of motion is allowed between the first and second set of tarsal bones. The ligaments of the ankle-joint are disposed as in the human subject, one at the inner and three at the outer side.

The ligaments that connect the metatarsal bone of the thumb to the internal cuneiform bone, are two in number, one at the upper and the other at the lower or plantar aspect; these limit the motions of flexion and extension, but allow very freely abduction and adduction. From this circumstance when the *peroneus longus* acts on the foot in turning the sole outwards, its tendency to bend the metatarsal bone upon the foot is resisted, and this bone is rendered a fixed point without the necessity of the counteraction of a muscular antagonist.

FRIDAY-EVENING PROCEEDINGS AT THE ROYAL INSTITUTION OF GREAT BRITAIN.

May 27.—Mr. Britton: Remarks on and Illustrations of the Old Domestic Architecture of England.

In the library, amongst many other things, were some Davy Protectors taken from the bottom of the Magicienne, now in dock at Woolwich. As usual, the iron had been removed to a great extent by the action of the salt water, aided by voltaic influence, and had left its bulk of that soft aggregate of plumbago, silicium, &c. remaining under such circumstances. But it was curious to remark that the mass

mass was impregnated with a substance resembling oil, which had been formed by the union of the carbon of the iron with hydrogen and oxygen, probably during the action of the water.

June 3.—Mr. Ritchie on Electricity, as the probable cause of all the phænomena of artificial and terrestrial magnetism.—Mr. Ritchie's object was to bring forward a connected and illustrated view of what had been done by Biot, Ampère, Barlow and others, in support of the hypothesis here announced.

June 10.—Mr. Faraday on the arrangements of particles on the surfaces of vibrating elastic bodies.—This was the same subject as that of the paper, by Mr. Faraday, read lately to the Royal Society; and though treated experimentally and very differently at the Royal Institution, yet as the philosophy of the two communications is the same, a reference to our report of that paper, which will be found at p. 42, will be sufficient here.

Mr. Faraday announced that since the reading of his paper, he had reason to believe that the principles there referred to, combined with the cohesive force of fluids, would enable him to explain the crispations that form on water lying upon similar vibrating plates. He is now engaged in these experiments.

This was the concluding evening of the Session.

VI. *Intelligence and Miscellaneous Articles.*

ACCOUNT OF AN AËRIAL VOYAGE MADE IN A BALLOON ON SATURDAY THE 30TH OF APRIL 1831, BY T. FORSTER, M.B. F.L.S. &c.

ON Saturday, April 30, 1831, Dr. Forster, who had long been desirous of pursuing his observations on the clouds in the lofty regions of the air, engaged to ascend with Mr. Green's balloon; and at half-past five o'clock in the evening, the air being calm and fine, and the barometer standing at 29·29, thermometer 63°, wind variable and gentle, the aëronauts proceeded to the gardens of the Dominican Friars at Moulsham near Chelmsford, and at a quarter before six they left the ground, amidst the huzzas of hundreds of spectators. The balloon, which was forty eight feet in vertical and thirty two in horizontal diameter, and filled with carburetted hydrogen from the gas-works, rose at first with a gentle motion, and was carried by a mild easterly breeze over the village of Writtle. At the elevation of about a thousand feet they hung out the anchor, which is found to give additional steadiness to the car; in a few minutes more they perceived the balloon checked in its velocity, and a change of current was evident; by this current they were carried nearly back again, the balloon still ascending and proceeding, though very slowly, in a remarkably gentle S.W. current: when they had got beyond the N.E. end of Chelmsford, and at the elevation of probably about four thousand feet, when nearly over the Convent of New Hall, the current again changed; they threw out some more ballast, and the balloon began to mount rapidly in a sort of irregular spiral course, but so gently as to be

scarcely perceived to move; till at length, at the altitude of nearly six thousand feet, it became perfectly motionless, and so remained for almost a quarter of an hour. Dr. Forster describes the sensation, at this period, to be most delightful;—balanced in the air, under a floating and inflated bag, in a perfectly calm and tranquil region, among the grotesque forms of evaporating clouds, and viewing in delicious tranquillity a vast and apparently concave panorama of country, bounded on one side by the sea, and everywhere interspersed with towns and villages, and the yellow and varied tint of the fields, the aërial travellers enjoyed a temporary repose from the noise and bustle of the world, which is seldom felt on the surface of the earth. More ballast being thrown out, the balloon again rose, and Dr. Forster now felt a disagreeable sensation, like pressure on the tympanum of the ears, just like what Garnerin, MM. Charles and Roberts, and others have described; in consequence of which he determined to open the valve, and they rapidly descended again into an inferior current, which carried them to Broomfield, where they eventually landed at twenty minutes before seven o'clock.

During, and subsequently to this aërial voyage, Dr. Forster made and recorded the following observations:

1st. That the balloon, when rising gently, gyrated in the same direction as the earth and planets do in their diurnal rotatory motion, that is from right to left: this motion was however so gentle, that it was only to be ascertained by observing objects below; in coming down the balloon oscillated in the same direction.

2ndly. The currents of air which they successively met with in ascending, came down during the next day in the same order of succession: the S.W. wind for example, into which they got, came down first on the following morning, and brought the rain. From repeated experiments, Dr. Forster has reason to believe this to be the case with most of the upper currents of air.

3rdly. The wavy cirrocumulus clouds are far beyond the reach of all balloons, and even when the clouds are seen from the greatest altitudes they seem as much above the ordinary clouds as they do above the earth.

4thly. Dr. Forster in comparing his elevation in the balloon to what he recollects of his Alpine ascents over the high Swiss mountains, is induced to account for the less degree, and in his own case absence, of giddiness experienced in balloons, to the idea of complete insulation: as when a person hangs over precipices, or sits on pinnacles, the notion of unsafe terrestrial attachment is the cause of the giddiness. Dr. Forster noticed, during the voyage, the manner in which clouds subside in an evening: he has also made some observations on the peculiar effects produced by different circumstances of sailing in boats at sea, and has compared them with those produced by aërial floating, which he purposes, before long, to communicate to the public; as also some physiological observations on the sea sickness which some persons feel; and on the very peculiar deafness experienced at great elevations, in diving bells, in mines, and on

change

change of weather, when the barometer rises or falls rapidly. One remarkable observation which Dr. Forster made, was, that on the occasion of descents from mountains, the deafness felt has always been accompanied by a sense of fullness about the ears; but this very unpleasant accompaniment of the difficulty of hearing was not felt after the descent from the balloon; the deafness in the latter case being simply dullness of hearing. In both cases it soon goes off. The sensation which is experienced when up in the higher regions of the air is not actually deafness, but a snapping in the ears; which goes off as one begins to descend; and the deafness which comes on in descending still lower, and which is worse on arriving at the ground, seems to be a sort of counterpart to the sensation experienced aloft. This very curious subject deserves further investigation.

With respect to giddiness and the fear of any danger from the sudden falling of the balloon, Dr. Forster observes, that having been accustomed to mount to very great altitudes, he felt neither of these disagreeable annoyances; but observed that on looking directly down, at the grapple hanging below, and on the ground beneath, he felt a disagreeable sensation, scarcely amounting to vertigo; but such as would have become so in persons not accustomed to great heights; and he therefore recommends aërial travellers to keep their eyes on the horizon rather than the ground directly below them, particularly if the car be large enough to admit of the choice.

The basket in which Dr. Forster ascended was a small one constructed for the purpose of lightness; but he is constructing a light circular willow basket capable of holding a proper set of instruments, in order to be ready, should a fit opportunity for another ascent occur.

P.S. Two parhelia were seen at five o'clock on Monday evening, May 9th, at Chelmsford.

LIST OF NEW PATENTS.

To J. Revere, Weybridge, Surrey, doctor of medicine, for a new and improved method of protecting iron chain cables, iron boilers, and iron tanks, from the corrosion produced on them by the action of water.—27th of November.—2 months allowed to enrol specification.

To W. Church, of Haywood House, Warwickshire, esq., for certain improvements in apparatus applicable to propelling boats and driving machinery by the agency of steam, parts of which improvements are also applicable to the purposes of evaporation.—29th of November, —5 months.

To R. Dalglish, junior, Glasgow, calico-printer, for improvements in machinery or apparatus for printing calicos and other fabrics.—6th of December.—6 months.

To H. Blundell, Kingston-upon-Hull, merchant, for improvements in a machine for grinding or crushing seeds and other oleaginous

ginous substances, for the purpose of abstracting oil therefrom, and which machine, with certain improvements or alterations, is applicable to other useful purposes.—6th of December.—6 months.

To R. Edwards, Dewsbury, Yorkshire, leather and flock seller, for an improvement on, or substitute for, glass, sand, emery, and other scouring-paper or substances.—6th of December.—6 months.

To S. Brown, Billiter-square, London, Commander in the Royal Navy, for certain improvements in the means of drawing up ships and other vessels from the water on land, and for transporting or mooring ships, vessels, and other bodies, on land, from one place to another.—6th of December.—6 months.

To J. G. Lacy, Camomile-street, London, gun-manufacturer; and S. Davis, East Smithfield, Middlesex, gun-lock maker, for a certain improvement or improvements in the construction of guns and fire-arms.—6th of December.—6 months.

To J. Dixon, Wolverhampton, and J. Vardy, of the same place, for certain improvements in cocks for drawing off liquids.—13th of December.—2 months.

To T. Walmsley, Manchester, manufacturer, for improvements in the manufacture of cotton, linen, silk, and other fibrous substances, into a fabric or fabrics applicable to various useful purposes.—13th of December.—6 months.

To W. Needham, Longour, Staffordshire, gentleman, for certain improvements in machinery for spinning, doubling, and twisting, silk and other fibrous substances.—13th of December.—6 months.

To S. Parlour, Croydon, Surrey, gentleman, for certain improvements on lamps, which he denominates Parlour's Improved Table Lamps.—13th of December.—2 months.

To J. L. Benham, Wigmore-street, Middlesex, ironmonger, for certain improvements on shower and other baths. Communicated by a foreigner.—13th of December.—6 months.

To R. Witty, Basford, in the parish of Wolstanton, Staffordshire, engineer, for certain improvements in apparatus for propelling carriages, boats, or vessels, and for other purposes, by the power of steam.—13th of December.—6 months.

To B. Redfern, Birmingham, gun-maker, for a lock, break-off, and trigger, upon a new and improved principle, for fowling-pieces, muskets, rifles, pistols, and small fire-arms of all descriptions.—Dated the 17th of December 1830.—2 months.

To A. Graham, a citizen of the United States of North America, but now residing in West-street, Finsbury, London, gentleman, for certain improvements in the application of springs to carriages. Communicated by a foreigner.—17th of December.—6 months.

To D. Papps, Stanley End, in the parish of King Stanley, Gloucestershire, machine-maker, for certain improvements in machinery for dressing or roughing woollen cloths.—23rd of December.—2 months.

To W. Wood, Summer Hill, Northumberland, near Newcastle-upon-Tyne, for the application of a battering-ram to the purpose of working coal in mines.—23rd of December.—4 months.

To

To M. E. A. Pertius, No. 56, Rue du Bac, Paris, spinster, for the fabrication or preparation of a coal fitted for refining and purifying sugar and other matters. Communicated by a foreigner.—23rd of December.—6 months.

To J. Ferrabee, Thrupp Mill and Foundry, Stroud, Gloucestershire, engineer, for improvements in the machinery for preparing the pile or face of woollen or other cloths requiring such a process. —23rd of December.—6 months.

To J. Blackwell and T. Alcock, both of Claines, Worcestershire, machine-makers, and lace or bobbin-net manufacturers, for certain improvements in machines or machinery for making lace, commonly called bobbin-net.—13th of January, 1831.—6 months.

To S. Seaward, Canal Iron Works, in the parish of All Saints, Poplar, engineer, for an improvement or improvements in apparatus for œconomising steam and for other purposes, and the application thereof to the boilers of steam-engines employed on board packet-boats and other vessels.—15th of January.—6 months.

To W. Parker, Albany-street, Regent's Park, gentleman, for certain improvements in preparing animal charcoal.—15th of January.—4 months.

To J. and G. Rodgers, Sheffield, cutlers; and T. Fellows, jun. New Cross, Deptford, gentleman, for an improved skate.—18th of January.—2 months.

To A. Smith, Princes-street, Leicester-square, St. Martin's in the Fields, engineer, for certain improvements in machinery for propelling boats and other vessels on water, and in the manner of constructing boats or vessels for carrying such machinery.—22nd of January.—6 months.

To J. G. Ulrich, Nicholas Lane, London, chronometer-maker, for certain improvements in chronometers.—22nd of January.—18 months.

LUNAR OCCULTATIONS.

Occultations of Planets and fixed Stars by the Moon, in July 1831. *Computed for Greenwich, by* THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1831.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean solar time.	Immersions. Angle from North Pole.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean solar time.	Emersions. Angle from North Pole.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
July 20	ς Ophiuchi	6	1944	19 27	11 35	55	79	20 32	12 40	295	326
24	π Capricorni	5	2403	19 17	11 9	110	100	20 37	12 29	272	275
31	ξ^2 Ceti	5	255	20 8	11 33	127	88	21 2	12 26	273	234

Results

Results of a Meteorological Journal for the Year 1830, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

By William Burney, *LL.D.*

Latitude 50° 47′ 45″ North: Longitude 1° 7′ West of Greenwich—In time $4^m\ 28^s$.

1830.	Barometer.										Six's Thermometer.								Mean Temp. of Spring Water.	De Luc's Hygrometer.						
	Max.	Min.	Media.	Range.	No. of Changes.	Spaces described.	Greatest Variation in 24 hours.	Media at 8 A.M.	Media at 2 P.M.	Media at 8 P.M.	Max.	Min.	Media.	Mean Range.	Gt. Var. in 24 hours.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.		Max.	Min.	Mean Range of the Index.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Media at 8,2,& 8 o'cl.
	In.	In.	In.	In.		In.	In.	In.	In.	In.	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Jan.	30·59	28·60	29·969	1·99	22	7·06	1·05	29·972	29·956	29·970	44	17	32·98	27	21	35·42	31·42	33·48	48·15	96	56	40	79·5	82·8	82·3	81·5
Feb.	30·33	29·43	29·910	0·90	23	5·00	0·44	29·902	29·913	29·918	53	13	36·07	40	26	39·07	33·53	35·36	46·95	98	58	40	76·6	82·7	87·5	82·3
Mar.	30·62	29·50	30·123	1·12	15	6·40	0·64	30·129	30·124	30·118	62	34	46·01	28	23	51·10	43·74	45·00	47·23	96	46	50	66·8	77·7	81·3	75·3
April	30·26	29·26	29·807	1·00	20	6·68	0·84	29·797	29·801	29·819	66	28	49·73	38	22	54·10	48·17	47·80	47·86	100	48	52	67·8	75·7	81·7	75·1
May	30·37	29·29	29·907	1·08	16	5·18	0·33	29·909	29·911	29·907	71	41	56·11	30	24	61·80	56·10	54·22	48·51	96	49	47	59·0	64·7	72·2	65·3
June	30·15	29·38	29·860	0·77	24	4·59	0·51	29·857	29·869	29·861	72	45	57·88	27	19	62·63	57·23	56·13	49·80	96	50	46	65·0	71·9	77·2	71·3
July	30·35	29·44	30·003	0·91	17	5·06	0·46	30·001	30·013	30·004	84	50	63·45	34	20	68·39	62·84	61·74	51·02	98	50	48	65·1	72·0	79·6	72·2
Aug.	30·32	29·43	29·950	0·89	19	5·31	0·50	29·954	29·951	29·945	75	44	59·79	31	21	65·19	58·68	58·90	52·77	91	48	43	59·8	66·9	74·0	66·9
Sept.	30·40	29·25	29·821	1·15	24	6·96	0·46	29·813	29·821	29·826	66	42	56·05	24	18	61·20	54·77	54·53	53·32	98	50	48	57·4	67·9	73·5	66·3
Oct.	30·51	29·76	30·235	0·75	18	4·26	0·43	30·235	30·233	30·236	65	36	53·24	29	21	57·90	51·29	52·39	53·32	95	57	38	65·8	75·3	77·0	72·7
Nov.	30·40	29·10	29·833	1·30	14	7·70	0·57	29·835	29·824	29·832	59	34	48·07	25	16	51·20	45·90	47·80	52·96	93	61	32	70·5	78·7	77·5	75·6
Dec.	30·45	28·86	29·607	1·59	22	8·13	0·72	29·607	29·619	29·610	52	16	38·26	36	20	41·13	36·29	38·81	51·58	92	59	33	74·4	80·8	78·7	78·0
Aver.	30·62	28·60	29·918	13·45	234	72·33	1·05	29·917	29·919	29·920	84	13	49·80	30·75	26	54·09	48·33	48·85	50·29	100	46	43·1	68·2	73·8	78·5	73·5

TABLE (continued).

1830.	Scale of the Winds.									Modifications of Clouds.							Weather.						Atmospheric Phænomena.									Evaporation in Inches, &c.	Rain in Inches, &c.
	North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	A clear Sky.	Fair, with Clouds.	An overcast Sky.	Foggy.	Rain, &c.	Total Number of Days.	Anthelia.	Parhelia.	Paraselenæ.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.		
January ..	7	12½	2	3	...	½	2	4	31	4	2	30	...	1	9	19	3	6	14	1½	6½	31	...	...	...	...	1	...	...	...	...	0·78	2·810
February	2½	5	2½	3	1½	9	2	2½	28	16	10	27	2	10	12	16	3½	9	10	2½	3	28	...	...	...	4	4	...	...	1	1	0·72	1·245
March ...	1	1	3	6½	2½	8	6½	2½	31	21	9	24	4	7	12	9	7½	12	9	...	2½	31	...	...	...	3	3	...	6	...	...	2·10	0·620
April	...	4	2	3½	½	11	3	6	30	18	12	24	...	18	19	17	6	13	5	...	6	30	...	5	4	3	1	1	4	1	1	2·70	3·235
May	3½	2	4	5	½	9	4½	2½	31	26	15	25	1	27	22	14	3½	17	6	...	4½	31	...	...	...	4	...	...	4	2	1	4·80	1·940
June	4½	1	1½	5	½	6½	7	4	30	24	13	30	...	24	25	19	3	13	8½	...	5½	30	...	3	2	1	2	...	...	1	1	3·40	2·630
July	½	2½	1½	3½	1	12½	7	2½	31	20	14	27	1	22	18	16	4½	15	6½	...	5	31	...	...	...	1	...	2	8	2	...	4·45	1·950
August ..	1½	1½	½	2	1	10½	8	6	31	22	11	30	1	24	29	20	3	16½	6	...	5½	31	...	...	...	3	1	...	9	4	1	3·50	3·400
September	2½	½	...	...	2	10	7½	7½	30	24	12	30	1	27	27	22	3½	16½	4½	...	5½	30	1	1	...	...	...	4	26	1	...	2·40	2·800
October ..	3	5½	4	3½	1	4	4½	5½	31	22	8	27	3	15	13	8	8	12	9	...	2	31	...	4	...	2	2	...	14	...	...	1·75	0·595
November	½	1	3	3½	4	13	2½	2½	30	21	9	28	1	20	21	21	1	15½	7½	...	6	30	1	1	...	2	3	3	10	...	...	1·20	4·695
December	5½	5½	3	3½	1½	2½	3	6½	31	16	4	31	...	14	10	18	2½	12	10½	1	5	31	...	...	...	1	4	4	9	...	...	0·80	2·430
Results for 1830.	32	42	27	42	16	96½	57½	52	365	234	119	333	14	209	217	199	49	157½	96½	5	57	365	2	14	6	24	21	14	90	12	5	28·60	28·350

ANNUAL

ANNUAL RESULTS FOR 1830.

Barometer.	Inches.
Greatest pressure of the atmosphere, March 26. Wind N.W.	30·620
Least ditto ditto January 20. Wind E.	28·600
Range of the quicksilver	2·020
Annual mean pressure of the atmosphere	29·918
Mean pressure for 175 days with the moon in North decl.	29·930
——— for 179 days with the moon in South decl.	29·951
Annual mean pressure at 8 o'clock A.M.	29·917
——— at 2 o'clock P.M.	29·919
——— at 8 o'clock P.M.	29·920
Greatest range of the quicksilver in January	1·990
Least range of ditto in October	0·750
Greatest annual variation in 24 hours in January	1·050
Least of the greatest variations in 24 hours in May	0·330
Aggregate of the spaces described by the rising and falling of the quicksilver	72·330
Number of changes	234·

Six's *Thermometer.*	Degrees.
Greatest thermometrical heat, July 29. Wind E.	84
——— cold, February 2. Wind N.E.	13
Range of the thermometer between the extremes	71
Annual mean temperature of the external air	49·80
——— at 8 A.M.	48·33
——— at 8 P.M.	48·85
——— at 2 P.M.	54·09
Greatest range in February	40·00
Least of the greatest monthly ranges in September	24·00
Annual mean range	30·75
Greatest monthly variation in 24 hours in February	26·00
Least of the greatest variations in 24 hours in November	16·00
Annual mean temperature of spring-water at 8 o'clock A.M.	50·29

De Luc's *Whalebone Hygrometer.*	Degrees.
Greatest humidity of the atmosphere, in April	100
Greatest dryness of ditto, in March	46
Range of the index between the extremes	54
Annual mean state of the hygrometer at 8 o'clock A.M.	73·8
——— at 8 o'clock P.M.	78·5
——— at 2 o'clock P.M.	68·2
——— at 8, 2, and 8 o'clock	73·5
Greatest mean monthly humidity of the atmosphere in Feb.	82·3
——— dryness of ditto in May	65·3

Position

Position of the Winds.	Days.
From North to North-east	32
—— North-east to East	42
—— East to South-east	27
—— South-east to South	42
—— South to South-west	16
—— South-west to West	96½
—— West to North-west	57½
—— North-west to North	52
	——365

Clouds, agreeably to the Nomenclature, or the Number of Days on which each Modification has appeared.

	Days.		Days.
Cirrus	234	Cumulus	209
Cirrocumulus	119	Cumulostratus	217
Cirrostratus	333	Nimbus	199
Stratus	14		

General State of the Weather.	Days.
A transparent atmosphere without clouds	49
Fair, with various modifications of clouds	157½
An overcast sky without rain	96½
Foggy	5
Rain, hail, and snow	57
	——365

Atmospheric Phænomena.	No.
Anthelia, or mock-suns opposite to the true sun	2
Parhelia, or mock-suns on the sides of the true sun	14
Paraselenæ, or mock-moons	6
Solar halos	24
Lunar halos	21
Rainbows	14
Meteors	90
Auroræ Boreales	17
Lightning, days on which it happened	12
Thunder, ditto ditto	5

Evaporation.	Inches.
Greatest monthly quantity in May	4·80
Least monthly quantity in February	0·72
Total amount for the year	28·60

Rain.	
Greatest monthly depth in November	4·695
Least monthly depth in October	0·595
Total amount for the year, near the ground	28·350

The instruments with which the observations were made are the same as those in former years, with the exception, that instead of the horizontal thermometer, a Six's thermometer has been used.

Barometrical Pressure.—The mean pressure this year is $\frac{7}{150}$ of an inch higher than the mean of the last fifteen years; but the number of changes is less than in any year in that period.

On the 19th of January a depression of 1·05 inch of mercury occurred, with a strong wind, first from South-east, and then from the North-east, accompanied with a heavy fall of snow.

The mean of the pressures at 8, 2 and 8 o'clock coincides with the annual mean pressure.

Temperature.—The mean temperature of the external air this year is lower than that of any year since 1816. The mean temperature of January and the first part of February was very low, and we have never registered so low a degree of temperature as that which occurred in the night of the 2nd of February. There is a difference of half a degree between the annual mean temperature at 8 A.M. and 8 P.M., the latter being the highest. The annual mean temperature of spring-water this year is the lowest during the last ten years.

Winds.—The duration of the South-west wind this year is unprecedented, being more than one-fourth of the period; the wind from the West is the next in duration; but that from the South has prevailed the least number of days.

The North-east and South-east winds are equal in point of time.

The number of strong gales of wind, or days on which they have prevailed this year, is as in the following scale:

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Gales.
4	12	6	7	6	45	15	8	103

January was very cold and cloudy, with frequent falls of snow; it having snowed more or less on fifteen days.

February was rather dry and mostly frosty, with a cloudy atmosphere, and occasional gales of wind. The *minimum* temperature in the night of the 2nd was the lowest that had occurred the last fifteen years.

March was fine, calm, and very dry, and a high pressure prevailed the latter part of the month, when the spring began to open.

April commenced with a snow-storm for several hours, and was alternately fine and showery, with frequent strong gales. In the night of the 4th the frost was severe, and did much injury to the bloom of the trees in open situations. Vegetation was checked the first part of the period by the cold nights and hoar frosts; but the warm rains, followed by clear sunny days during the latter part, caused a rapid growth.

May was dry and pleasant, with much warm sunshine till the 20th, when the air became arid and blighty, and the roads very dusty, insomuch that vegetation began to droop. The latter part of the month was showery, accompanied with strong gales.

June was cold, showery and windy. The crops of grass, from the nature of the weather in the spring, were generally abundant; but

but from a continuance of wet, much of the early cut grass was spoilt before it could be put in ricks sufficiently dry.

July was also cold, showery, and windy till the 12th; but the latter part of the month was fine and dry, with four or five hot days and nights, which matured the wheat.

August was fine and dry till the 8th, during which time much exertion was made by the agriculturists in securing the wheat crops. The remainder of the month was showery and windy; and as scarcely any good opportunity occurred to get in the wheat in a dry state, much was lost out of the standing sheaves in the fields before it could be carried.

September was wet and windy, which occasioned difficulties in getting in the barley and oat crops, both of which were abundant, and far superior in quality to those of several years past. The fruit crops were also abundant, but they were generally deficient in natural flavour, and mostly worm-eaten.

October was very dry, calm, and fine, with a high atmospheric pressure.

November was boisterous and wet, which caused floods in many places; but the air was mild for the season, with the exception of a few days.

December was also wet and windy, and after the 10th, cold and frosty, with snow at intervals, and a low pressure.

The frequency of auroræ boreales in the autumn was remarkable: fair descriptions of their appearances may be seen under the monthly meteorological reports in this work.

Extract from the Meteorological Journal kept at Penzance by Mr. GIDDY.

ANNUAL RESULTS.

1830.	Barometer.			Register Thermometer.			Rain in Inches.	Wet Days.	Dry Days.	Prevailing Winds.
	Max.	Min.	Mean.	Max.	Min.	Mean.				
Jan.	30·50	29·00	29·970	48	23	37·0	2·480	15	16	NE.
Feb.	30·20	29·45	29·877	53	19	40·0	2·510	17	11	W.
Mar.	30·32	29·35	30·018	61	39	48·8	1·635	10	21	NW.
April	30·10	29·20	29·735	62	32	49·5	5·085	15	15	W.
May	30·25	29·30	29·814	65	42	54·5	2·415	12	19	SE.
June	30·10	29·30	29·830	67	45	56·7	3·225	14	16	N.
July	30·20	29·32	29·895	76	49	60·5	1·930	8	23	NW.
Aug.	30·25	29·40	29·835	68	47	58·3	4·370	17	14	NW.
Sept.	30·30	29·25	29·805	64	45	55·4	6·030	18	12	NW.
Oct.	30·40	29·82	30·120	62	43	54·0	2·360	12	19	SE.
Nov.	30·30	28·95	29·730	58	39	49·5	5·120	16	14	SW.
Dec.	30·40	28·60	29·625	53	25	43·0	5·315	21	10	NW.
1830	30·50	28·60	29·8546	76	19	50·6	42·475	175	190	NW.
1829	30·50	28·85	29·8710	71	19	50·4	43·045	182	183	NW.

The rain-gauge is at the ground-level, and the dry days comprehend those days on which no fall whatever takes place,—not the slightest shower.

METEORO-

METEOROLOGICAL OBSERVATIONS FOR MAY 1831.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·303. May 9. Wind N.E.—Min. 29·478. May 1. Wind S.
Range of the mercury 0·825.
Mean barometrical pressure for the month .... 29·920
Spaces described by the rising and falling of the mercury........... 4·096
Greatest variation in 24 hours 0·286.—Number of changes 22.
Therm. Max. 72°. May 25. Wind S.E.—Min. 35°. May 6. Wind N.
Range 37°.—Mean temp. of exter. air 54°·76. For 31 days with ☉ in ♉ 52·85
Max. var. in 24 hours 20°·00.—Mean temp. of spring-water at 8 A.M. 49·60

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 29th.... 94°
Greatest dryness of the atmosphere, in the afternoon of the 18th... 43
Range of the index 51
Mean at 2 P.M. 57°·5.—Mean at 8 A.M. 62°·7.—Mean at 8 P.M. 68·5
—— of three observations each day at 8, 2, and 8 o'clock .... 62·9
Evaporation for the month 6·35 inches.
Rain in the pluviameter near the ground 2·07 inches.
Prevailing winds, S.E. and N.E.

Summary of the Weather.

A clear sky, 5½; fine, with various modifications of clouds, 15½; an overcast sky without rain, 4½; rain, 5½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
25	10	25	0	22	15	13

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3½	8	4	8½	½	4½	1	1	31

General Observations.—This month has been alternately wet and dry; the dry period was from the 5th to the 19th, during which time no rain fell here, and the air was frequently arid with a North-east wind, accompanied with a great evaporation.

For several days upwards of three-tenths of an inch evaporated, and on the 18th, the day before the rain came on, half an inch in depth ascended from the evaporator, which clearly indicates the greater power of the air in eliciting heat, and carrying off so much moisture from near the surface of the earth immediately before the change from dryness to rain, and which was verified by the hygrometer's, then pointing out the maximum dryness for the month.

There were several hoar frosts in the first part of the month, when great blights prevailed, and their effects have since been made manifest by the trees being much thinned of their fruit. Comparatively speaking there were but a few warm days this month, and the mean temperature of the external air is about one degree under the mean of May for many years past.

On the 5th, at three P.M. a heavy flash of lightning was experienced here, and was succeeded by a loud clap of thunder; distant thunder also frequently occurred in the morning of the 24th.

At half-past ten o'clock in the night of the 30th, a few light coruscations ascended due North from an aurora borealis, which was low in the Northern horizon, and two meteors fell over it. This may be considered late in the spring for its appearance, as the evening twilight in that quarter is getting long, and being of a similar appearance to that of an aurora, it renders it imperceptible in the summer months.

The

The planet Mercury was seen here with the naked eye in the fine evenings, from the 1st to the 13th; and Venus was seen at noon of the 16th, notwithstanding she is still in the superior part of her orbit.

The atmospheric and meteoric phænomena that have come within our observations this month, are one solar and two lunar halos, two meteors, one rainbow, one aurora borealis, lightning and thunder on the 5th, distant thunder on the 24th, and four gales of wind, namely, two from the North-east, and two from the East.

Occultation of Jupiter and his Satellites by the Moon.—The following were the apparent astronomical times here, on June the 1st, when the immersions and emersions of Jupiter and his Satellites occurred, with the exception of the 4th Satellite, the times of whose immersion and emersion are given by approximation.

IMMERSIONS.		EMERSIONS.	
The 4th Satellite at...	$12^h 40^m 48^s$	The 4th Satellite at ...	$13^h 49^m 6^s$
The 1st Satellite at...	12 57 50	The 1st Satellite at ...	14 6 10
Jupiter's western limb at	12 59 49	Jupiter's western limb	14 7 50
Jupiter's centre at	13 1 45	Jupiter's centre at......	14 9 47
Jupiter's eastern limb at	13 3 42	Jupiter's eastern limb at	14 11 45
The 2nd Satellite at...	13 6 10	The 2nd Satellite at ...	14 12 25
The 3rd Satellite at...	13 10 41	The 3rd Satellite at ...	14 17 43

The occultation of Jupiter lasted 1 hour 8 minutes 3 seconds. From the motion of the Satellites the times of their occultations do not exactly agree with that of their primary.

Jupiter was not so nicely dichotomized on the Moon's enlightened limb as on her dark side: and after his emersion he appeared with an apparently enlarged disc, and shone more brilliantly than before his immersion, as well as his Satellites.

REMARKS.

London. — May 1. Cloudy: showers. 2. Heavy rain, with thunder. 3. Showers: fine at night. 4. Showers, with intervals of bright sun. 5. Fine in the morning: showers: clear and cold at night. 6. Fine: cloudy: clear at night, with severe frost for the advanced state of vegetation. 7, 8. Fine: frosty at nights. 9—13. Fine, but cold and very dry. 14. Fine: slight frost at night. 15, 16. Fine. 17, 18. Very dry. 19. Fine: rain at night. 20. Cloudy: heavy thunder shower in the afternoon. 21, 22. Overcast. 23. Cloudy: thunder in the afternoon. 24. Fine: dull and hazy, with thunder at night. 25—28. Fine. 29. Wet. 30. Cloudy: fine. 31. Foggy in the morning: fine.

Penzance.—May 1. Fine. 2, 3. Clear: showers. 4, 5. Fair: hail showers. 6—8. Fair. 9, 10. Clear. 11. Fair. 12, 13. Clear. 14, 15. Fair. 16. Clear. 17. Fair. 18. Clear: a shower. 19. Fair: rain. 20. Fair. 21. Clear. 22. Fair. 23. Fair: foggy. 24. Foggy: fair. 25. Fair. 26. Fair: rain at night. 27. Rain. 28. Fair: rain. 29. Rain. 30. Fair: rain. 31. Rain.

Boston. — May 1. Fine. 2. Cloudy. 3. Rain. 4. Cloudy: rain A.M. and P.M. 5. Fine: rain P.M. 6, 7. Fine. 8. Cloudy. 9—12. Fine. 13—15. Cloudy. 16—19. Fine. 20, Cloudy: shower P.M. 21. Cloudy. 22. Fine. 23. Cloudy: rain, with thunder and lightning early A.M. 24. Fine. 25. Cloudy. 26. Fine. 27—30. Cloudy. 31. Fine.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1831.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
May 1	29·534	29·452	29·35	29·30	29·496	29·478	28·88	67	40	61	49	62	51	58·5	S.	SE.	SW.	calm	...	0·04	...	0·255	...
2	29·609	29·546	29·50	29·45	29·603	29·509	29·10	61	47	61	47	64	52	50	E.	SE.	SE.	calm	...	·72	...	·080	...
3	29·681	29·653	29·60	29·60	29·708	29·626	29·06	62	48	62	49	61	50	50	SW.	SW.	SW.	calm	0·30	·12	0·115	·040	...
4	29·597	29·568	29·62	29·60	29·700	29·568	29·02	66	39	55	46	58	44	56	S.	N.	SW.	S.	...	·05	...	·130	0·06
☾ 5	29·625	29·544	29·65	29·60	29·684	29·553	29·01	66	38	52	43	56	39	52	S.	NW.	W.	calm	...	·12	·255	·175	·36
6	29·947	29·784	29·85	29·85	29·933	29·836	29·22	51	28	53	43	53	35	42·5	NW.	NE.	N.	calm	·40	...	...	...	·15
7	30·357	30·043	29·90	29·90	30·113	30·040	29·56	55	33	54	36	51	38	44	E.	NE.	SE.	W.	...	...	...	...	...
8	30·284	30·255	30·20	30·00	30·292	30·191	29·82	51	35	56	43	56	39	47	E.	E.	NE.	calm	...	...	...	...	...
9	30·347	30·214	30·18	30·10	30·303	30·217	29·90	59	39	55	40	56	43	49·5	E.	NE.	NE.	calm	·80	...	...	...	...
10	30·234	30·191	30·00	29·95	30·148	30·127	29·78	56	38	56	44	56	39	53	NE.	NE.	NE.	E.	·35	...	...	...	...
11	30·205	30·201	30·00	30·00	30·195	30·158	29·75	61	33	56	44	59	43	53	E.	E.	NE.	E.	·30	...	...	...	...
● 12	30·137	30·010	30·05	30·02	30·143	30·050	29·64	67	35	63	44	60	45	55	SE.	NE.	SE.	W.	·30	...	...	...	...
13	30·059	29·949	29·90	29·90	30·023	29·960	29·50	66	40	65	45	60	43	50·5	NE.	NE.	NE.	calm	·35	...	...	...	...
14	30·097	30·063	29·90	29·90	30·106	30·077	29·60	67	31	56	48	56	39	47	E.	SE.	NE.	calm	·35	...	...	...	...
15	30·074	30·025	29·92	29·92	30·088	30·073	29·60	63	37	60	44	55	42	55	S.	SE.	S.	calm	·35	...	...	...	...
16	30·201	30·139	30·00	30·00	30·204	30·170	29·62	73	37	61	43	60	47	55	N.	SE.	SE.	NW.	·25	...	...	...	...
17	30·212	30·118	30·00	29·95	30·208	30·089	29·60	74	41	64	49	62	53	61·5	E.	SE.	E.	calm	·40	...	...	...	...
☽ 18	30·033	29·886	29·85	29·65	30·016	29·848	29·52	70	52	66	50	65	54	61·5	E.	SE.	SE.	E.	·50	...	...	...	...
19	29·775	29·720	29·60	29·55	29·730	29·651	29·33	68	53	57	54	64	55	60	E.	SE.	E.	SE.	·20	·40	·565	·090	...
20	29·836	29·642	29·80	29·60	29·868	29·798	29·30	75	49	65	52	67	56	60	S.	SW.	SE.	calm	·10	·11	...	·100	...
21	29·993	29·916	30·00	29·90	29·977	29·877	29·41	69	51	63	50	66	53	58	E.	NW.	N.	calm	·15	...	...	·070	...
22	30·004	29·964	30·00	30·00	30·030	29·995	29·50	69	50	60	49	68	56	61	N.	NW.	N.	N.	·20	...	...	...	...
23	30·000	29·845	29·90	29·80	29·907	29·857	29·32	77	54	62	49	69	57	56	N.	NW.	SW.	NE.	·20	...	...	...	·41
24	29·833	29·831	29·78	29·70	29·819	29·807	29·37	78	53	65	52	69	55	63	NW.	S.	NE.	E.	...	...	...	·070	...
25	29·871	29·840	29·80	29·70	29·862	29·811	29·37	75	47	66	54	72	56	61	NE.	SE.	SE.	W.	...	...	...	...	...
○ 26	29·843	29·779	29·73	29·65	29·827	29·775	29·38	72	49	63	53	66	53	57	NE.	SE.	E.	NE.	...	...	...	·010	...
27	29·825	29·785	29·75	29·70	29·790	29·780	29·48	67	49	57	54	57	51	54	E.	SE.	NE.	calm	·45	...	·720	·400	...
28	29·987	29·925	29·80	29·80	29·993	29·900	29·51	70	50	64	52	61	53	51·5	S.	S.	N.	calm	...	...	·210	·210	...
29	29·954	29·785	29·82	29·80	29·927	29·868	29·55	56	49	64	53	65	53	55	SE.	SE.	SE.	calm	...	·65	·120	·440	...
30	29·969	29·899	29·82	29·80	29·930	29·910	29·45	68	46	64	49	64	49	57	E.	SE.	SE.	calm	...	...	·190	...	...
31	29·985	29·897	29·84	29·84	29·977	29·870	29·54	67	46	58	55	63	51	57	NE.	SE.	E.	E.	·40	...	·265	...	...
	30·357	29·452	30·20	29·30	30·303	29·478	29·44	78	28	66	36	72	35	54·5					6·35	2·21	2·440	2·070	0·98

THE

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[NEW SERIES.]

AUGUST 1831.

VII. *On some Problems in Analytical Geometry.* By J. W. Lubbock, *Esq. V.P. & Treas. R.S.**

Let the equation to any curve surface of the second order be

$$Ax^2+Cy^2+Kz^2+Bxy+Lzy+Mxz+Dx+Ey+Nz+F=0,$$
$$(F\,(x,y,z)=0)$$

the coordinate axes x, y, z being inclined to each other at any angle, and let ϱ be the distance of any point (α, β, γ) from (x, y, z), (x, y, z) being situated on the curve surface.

$$\varrho^2 = (x-\alpha)^2 + (y-\beta)^2 + (z-\gamma)^2$$
$$+ 2(x-\alpha)(y-\beta)\cos xy + 2(y-\beta)(z-\gamma)\cos zy +$$
$$2(z-\gamma)(x-\alpha)\cos xz \qquad (1)$$

If ϱ is a maximum or minimum and λ is some indeterminate quantity,

$$\lambda\{2Ax+By+Mz+D\} = x-\alpha+(y-\beta)\cos xy+(z-\gamma)\cos xz \qquad (2)$$

$$\lambda\{2Cy+Bx+Lz+E\} = y-\beta+(x-\alpha)\cos xy+(z-\gamma)\cos xy \qquad (3)$$

$$\lambda\{2Kz+Ly+Mx+N\} = z-\gamma+(x-\alpha)\cos zx+(y-\beta)\cos zy \qquad (4)$$

Eliminating λ,

$$\{2Ax+By+Mz+D\}\{y-\beta+(x-\alpha)\cos xy+(z-\gamma)\cos zy\}$$
$$=\{2Cy+Bx+Lz+E\}\{x-\alpha+(y-\beta)\cos xy+(z-\gamma)\cos xz\} \qquad (5)$$

$$\{2Cy+Bx+Lz+E\}\{z-\gamma+(x-\alpha)\cos zx+(y-\beta)\cos zy\}$$
$$=\{2Kz+Ly+Mx+N\}\{y-\beta+(x-\alpha)\cos xy+(z-\gamma)\cos zy\} \qquad (6)$$

which are the equations to curve surfaces whose intersections determine the points from which the normals can be drawn from the point α, β, γ to the curve $F(x, y, z)$.

* Communicated by the Author.

Let $D=0$, $E=0$, $N=0$; then multiplying equations (2) (3) and (4) first by x, y and z respectively, then by $x-\alpha, y-\beta$ and $z-\gamma$ respectively, and then by α, β, γ respectively, and adding together the results, R being the distance of the point α, β, γ from the centre, and r the distance of the point in which the normal cuts the curve surface, from the centre,

$$\lambda = -\frac{\varrho^2 + r^2 - R^2}{4F}.$$

$$r^2 = x^2 + y^2 + z^2 + 2xy\cos xy + 2zy\cos zy + 2zx\cos zx$$
$$R^2 = \alpha^2 + \beta^2 + \gamma^2 + 2\alpha\beta\cos xy + 2\gamma\beta\cos zy + 2\gamma\alpha\cos zx$$

λ is therefore evidently independent of the direction of the axes x, y, z.

Eliminating x, y, z from the equations

$$\lambda\{2Ax + By + Mz\} = x + y\cos xy + z\cos xz$$
$$\lambda\{2Cy + Bx + Lz\} = y + x\cos xy + z\cos zy$$
$$\lambda\{2Kz + Ly + Mx\} = z + x\cos zx + y\cos zy$$

which obtain upon the supposition that $\alpha, \beta, \gamma = 0$, in which case $\lambda = -\frac{r^2}{2F}$.

$$r^6$$
$$-2F^2\left\{\frac{4KC + 4AC + 4AK + L^2 + B^2 + M^2 - 2BL\cos zx - 2BM\cos zy - 2ML\cos yx}{8AKC - 2AL^2 - 2KB^2 - 2CM^2 - 2MLB}\right\}r^4$$
$$+4F^2\left\{\frac{2C\sin^2 zx + 2K\sin^2 xy + 2A\sin^2 zy - 2L(\cos zy - \cos zx\cos xy) - 2B(\cos xy - \cos zx\cos zy) - 2M(\cos zx - \cos xy\cos zy)}{8AKC - 2AL^2 - 2KB^2 - 2CM^2 - 2MLB}\right\}r^2$$
$$-8F^3\left\{\frac{1 - \cos^2 zy - \cos^2 xy - \cos^2 zx + 2\cos zx\cos xy\cos zy}{8AKC - 2AL^2 - 2KB^2 - 2CM^2 - 2MLB}\right\} = 0$$

If m, n, p are the principal axes of the surface, the coefficient of $r^4 = m^2 + n^2 + p^2$

of $r^2 = m^2p^2 + m^2n^2 + n^2p^2$

and the quantity independent of $r = m^2n^2p^2$ because m^2, n^2, p^2 are evidently the *roots* of the equation.

If the curve surface be referred to conjugate axes m', n', p' so that

$$A = m'^2p'^2,\ C = m'^2n'^2,\ K = m'^2n'^2$$
$$B = 0,\ L = 0,\ M = 0,\ F = -m'^2n'^2p'^2$$

$$r^6 - (m'^2 + n'^2 + p'^2)r^4$$
$$+(p'^2n'^2\sin^2 yz + p'^2m'^2\sin^2 zx + m'^2n'^2\sin^2 xy)r^2$$
$$-m'^2n'^2p'^2(1 - \cos^2 zy - \cos^2 xy - \cos^2 zx$$
$$+2\cos zx\cos xy\cos zy) = 0.$$

Whence

Whence

$$m'^2 + n'^2 + p'^2 = m^2 + n^2 + p^2$$

$$p'^2 n'^2 \sin^2 y + p'^2 m'^2 \sin^2 z x + m'^2 n'^2 \sin^2 x y = p^2 n^2 + p^2 m^2 + m^2 n^2$$

$$m'^2 n'^2 p'^2 (1 - \cos^2 zy - \cos^2 xy - \cos^2 z x + 2 \cos zx \cos xy \cos zy) = m^2 n^2 p^2$$

which are the known properties of the conjugate diameters; the first and third of these theorems appear to have been first proved by M. Livet, in the thirteenth Number of the *Journal de l'Ecole Polytechnique;* and the second by M. Brianchon in a memoir "Sur la Théorie des Axes conjugués, et des Momens d'Inertie des Corps," *Journal de l'Ecole Polytechnique,* vol. viii. p. 65.

If x', y', z' are the coordinates of any point in the normal drawn from the origin, which coincides with the centre, of the curve surface,

$$Ax^2 + Cy^2 + Kz^2 + Bxy + Lzy + Mzx + F = 0$$

$$y = \frac{x y'}{x'}, \quad z = \frac{x z'}{x'}$$

$$(2Ax' + By' + Mz')(y' + x' \cos xy + z' \cos zy)$$
$$= (2Cy' + Bx' + Lz')(x' + y' \cos xy + z' \cos xz)$$

$$(2Cy' + Bx' + Lz')(z' + x' \cos zx + y' \cos zy)$$
$$= (2Kz' + Ly' + Mx')(y' + x' \cos xy + z' \cos zy)$$

which are equations to conical surfaces whose intersections are the principal axes of the curve surface,

$$Ax^2 + Cy^2 + Kz^2 + Bxy + Lzy + Mxz + F = 0.$$

Transferring the origin to the point α, β, γ

$$Ax^2 + Cy^2 + Kz^2 + Bxy + Lzy + Mxz + Dx + Ey + Nz + F = 0$$

$$D = -2A\alpha - B\beta - C\gamma$$
$$E = -2C\beta - B\alpha - L\gamma$$
$$N = -2K\gamma - L\beta - M\alpha$$

$$\{2Ax' + By' + Mz' + D\}\{y' - \beta + (x' - \alpha)\cos xy + (z' - \gamma)\cos zy\}$$
$$= \{2Cy' + Bx' + Lz' + E\}\{x' - \alpha + (y' - \beta)\cos xy + (z' - \gamma)\cos xz\}$$

$$\{2Cy' + Bx' + Lz' + E\}\{z' - \gamma + (x' - \alpha)(\cos zx + (y' - \beta)\cos zy\}$$
$$= \{2Kz' + Ly' + Mx' + N\}\{y' - \beta + (x' - \alpha)\cos xy + (z' - \gamma)\cos zy\}$$

In order to have the equations to these conical surfaces which determine the principal axes in the most general case possible, it is only necessary to substitute in the preceding

 equations

equations the values of α, β, γ, found by elimination, in terms of A, B, C, D, E, L, M, N from equations of lines 29, 30, 31.

If the equation to the curve surface be in the form $n^2 p^2 x^2 + m^2 p^2 y^2 + m^2 n^2 z^2 = m^2 n^2 p^2$, and α, β, γ are the coordinates of the point, as before, from which normals are drawn,

$$m^2 y (x-\alpha) = n^2 x (y-\beta)$$
$$n^2 z (y-\beta) = p^2 y (z-\gamma)$$
$$p^2 x (z-\gamma) = m^2 z (x-\alpha)$$

These are the equations to cylinders which have for their bases equilateral hyperbolas, and which pass through the centre of the curve surface and the point α, β, γ.

Let x', y', z' be the coordinates of any point in the normal drawn from the point α, β, γ to the point x, y, z in the curve surface,

$$\frac{x-\alpha}{y-\beta} = \frac{x'-\alpha}{y'-\beta} \qquad \frac{x-\alpha}{z-\gamma} = \frac{x'-\alpha}{z'-\gamma}$$

Eliminating x, y, z between these equations,

$$(\alpha y' - \beta x')^2 \{m^2 (x'-\alpha)^2 + n^2 (y'-\beta)^2 + p^2 (z'-\gamma)^2\}$$
$$= (n^2 - m^2)^2 (x'-\alpha)^2 (y'-\beta)^2$$
$$(\gamma x' - \alpha y')^2 \{m^2 (x'-\alpha)^2 + n^2 (y'-\beta)^2 + p^2 (n'-\gamma)^2\}$$
$$= (m^2 - p^2) (z'-\gamma)^2 (x'-\alpha)^2$$
$$(\beta x' - \gamma y')^2 \{m^2 (x'-\alpha)^2 + n^2 (y'-\beta)^2 + p^2 (z'-\gamma)^2\}$$
$$= (p^2 - m^2)^2 (y'-\beta)^2 (z'-\gamma)^2$$

which are the equations of conical surfaces whose intersections are the normals which can be drawn from the point α, β, γ to the curve surface.

The equation to the cone which circumscribes the surface

$$n^2 p^2 x^2 + m^2 p^2 y^2 + m^2 n^2 z^2 = m^2 n^2 p^2,$$

and whose vertex coincides with the point α, β, γ, is

$$n^2 p^2 (x-\alpha)^2 + m^2 p^2 (y-\beta)^2 + m^2 n^2 (z-\gamma)^2$$
$$- p^2 (\beta x - \alpha y)^2 - m^2 (\alpha z - \gamma x)^2 - n^2 (\gamma y - \beta z)^2 = 0;$$

and the equation to the plane of contact is

$$n^2 p^2 \alpha x + m^2 p^2 \beta y + n^2 m^2 \gamma z = m^2 n^2 p^2.$$

These equations relative to the circumscribing cone are given in various works on analytical geometry.

If the ellipse

$$n^2 x^2 + m^2 y^2 = m^2 n^2$$

be considered, and if the axes x and y, be at right angles,

$$\lambda x = m^2 (x-\alpha)$$
$$\lambda y = n^2 (y-\beta)$$

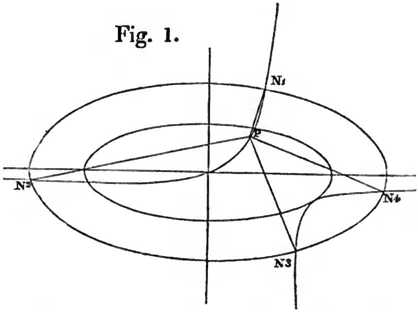
and

and eliminating λ,

$$(m^2-n^2)\,x\,y-m^2\,\alpha\,y+n^2\,\beta\,x=0$$

which is the equation to an equilateral hyperbola (see fig. 1.), which cuts the ellipse in the points to which normals can be drawn from the point α, β. The asymptotes to the hyper-

Fig. 1.



bola are parallel to the principal axes of the curve; it passes through the centre and the point (α, β); the coordinates of its centre are $\frac{m^2\,\alpha}{m^2-n^2}$ and $-\frac{n^2\,\beta}{m^2-n^2}$. When α or $\beta=0$, the hyperbola merges into two straight lines at right angles to each other; and when the point (α, β) coincides with the centre, these lines become the principal axes of the curve.

If the equation to the curve be $y^2=2\,p\,x$, the equation to the corresponding hyperbola is

$$x\,y-(\alpha-p)\,y+p\,\beta=0.$$

The centre is situated on the axis of the parabola at a distance $\alpha-p$ from the vertex. This construction is given by Apollonius. See Bossut, *Histoire des Mathématiques*, vol. i. p. 37.

If x' and y' be the coordinates of any point in the normal,

$$\frac{x'-\alpha}{y'-\beta}=\frac{x-\alpha}{y-\beta}$$

and eliminating x and y between this equation and the equations

$$n^2\,x^2+m^2\,y^2=m^2\,n^2;\ (m^2-n^2)\,x\,y-m^2\,\alpha\,y+n^2\,\beta\,x=0,$$

we have

$$\{m^2\,(x'-\alpha)^2+n^2\,(y'-\beta)^2\}\ \{\beta\,x-\alpha\,y\}^2-(m^2-n^2)^2\,(x'-\alpha)^2\,(y'-\beta)^2=0$$

which is the equation to the normals drawn from any point $\alpha\,\beta$.

When $m=n$, the ellipse becomes a circle: in this case

$\beta\,x-$

$\beta x - \alpha y = 0$, which is the equation to the line joining the centre of the circle and the point $\alpha \beta$.

It is evident that since the hyperbola

$$(m^2 - n^2) x y - m^2 \alpha y + n^2 \beta x = 0$$

is the same for all similar ellipses, there will be one, as fig. 1., which touches the hyperbola.

In this case two of the normals, as P N_3, P N_4, coincide and become equal; after this the ellipse diminishes, one branch of the hyperbola ceases to cut the ellipse, and only two normals can be drawn from the point $\alpha \beta$ to the ellipse.

When the ellipse touches the hyperbola, the values of $\frac{dy}{dx}$ are the same in both.

Let $n'^2 x^2 + m'^2 y^2 = n'^2 m'^2$ be the equation to this ellipse, then

$$\{(m^2 - n^2) x - m^2 \alpha\} n'^2 x = m'^2 y \{(m^2 - n^2) y + n^2 \beta\}$$

and since $\frac{n^2}{m^2} = \frac{n'^2}{m'^2}$

$$\{(m^2 - n^2) x - m^2 \alpha\} n^2 x = m^2 y \{(m^2 - n^2) y + n^2 \beta\}$$

and eliminating x and y between this equation and the equations

$$(m^2 - n^2) x y - m^2 \alpha y + n^2 \beta x = 0$$

and $$m^2 y^2 + n^2 x^2 = m^2 n^2$$

$$m^2 - n^2 = \{m^{\frac{2}{3}} \alpha^{\frac{2}{3}} + n^{\frac{2}{3}} \beta^{\frac{2}{3}}\}^{\frac{3}{2}}$$

the equation to the evolute of the ellipse.

And hence from any point within the evolute of an ellipse four normals can be drawn from any point to the curve, from any point without it only two.

On the Determination of the Foci.

Let $n^2 x^2 + m^2 y^2 = m^2 n^2$

and let the the curve be referred to any other coordinates x' and y' by putting

$$a (x' + \alpha') + b (y' + \beta') \text{ for } x$$

and $$a' (x' + \alpha') + b' (y' + \beta') \text{ for } y$$

$$A x'^2 + B x' y' + C y'^2 + D x' + E y' + F = 0$$

$A = n^2 a^2 + m^2 a'^2$, $B = 2 (n^2 a b + m^2 a' b')$, $C = n^2 b^2 + m^2 b'^2$

$D = 2 (n^2 a^2 \alpha' + n^2 a b \beta' + m^2 a^2 \alpha + m^2 a b \beta')$

$E = 2 (n^2 b^2 \beta' + n^2 b^2 \alpha' + m^2 b'^2 \beta' + m^2 b^2 \alpha')$

$F = n^2 a^2 \alpha'^2 + n^2 b^2 \beta'^2 + 2 n^2 \alpha' \beta' + m^2 b^2 \beta'^2 + 2 m^2 a' b' \alpha' \beta' - m^2 n^2$

Now let the equation $A x'^2 + B x' y' + C y'^2 + D x' + E y' + F = 0$ be

be transferred to polar coordinates by putting $r\,a''$ for x'' and $r\,b''$ for y', r being the distance of any point from the origin,

$$(A\,a''^2 + B\,a''\,b'' + C\,b''^2)\,r^2 + (D\,a'' + E\,b'')\,r + F = 0$$

$$r = \frac{\left\{\begin{array}{l} -D\,a''-E\,b' \\ \pm\,\{(D^2-4\,AF)\,a''^2 + 2\,(DE-4\,BF)\,a''\,b'' \\ \qquad + (E^2-4\,cF)\,b''^2\}^{\frac{1}{2}} \end{array}\right\}}{2\,(A\,a''^2 + B\,a''\,b'' + C\,b''^2)}$$

and since $a''^2 + b''^2 + 2\,a''\,b''\cos x\,y = 1$

$$r = \frac{\left\{\begin{array}{l} -D\,a''-E\,b'' \\ \pm\{\,D^2-4\,AF + 2(DE-2\,BF-D^2-4\,AF)\cos xy\}c'' \\ \qquad +\{E^2-4\,CF-D^2+4\,AF\}b'^2\}^{\frac{1}{2}} \end{array}\right\}}{2\,(A\,a''^2 + B\,a''\,b'' + C\,b''^2)}$$

which value of r will be rational in terms of a'' and b'' if

$$E^2 - 4\,C\,F - D^2 + 4\,A\,F = 0$$
$$D\,E - 2\,BF - (D^2 - 4\,A\,F)\cos x\,y = 0$$

putting for A, B, C, D, E and F their values above, it will be found, after many reductions, that these equations may be put in the form

$$(a'\,b - a\,b')^2\,(\beta'^2 - \alpha'^2) = (a^2 - b^2)\,m^2 - n^2)$$
$$(a'\,b - a\,b')^2\,(\alpha'\,\beta' + \beta'^2\cos x'\,y') = (n^2-m^2)\,(a^2\cos x'\,y' - a\,b)$$

In these equations α' and β' are measured from the center of the curve in the direction of the axes x' and y': the equations to the same curve, found either by similar substitutions for the quantities A, B, C, D, E, and F or by transforming the last two equations, referred to coordinate axes coinciding with the principal axes of the curve, are

$$(a'^2-b'^2)\,(\alpha^2-\beta^2) + 2\,(b\,b'-a\,a')\,\alpha\,\beta = (a'^2-b'^2)\,(m^2-n^2)$$
$$a\,a'\,(\alpha^2-\beta^2) + (a^2-b'^2)\,\alpha\,\beta = a\,a'\,(m^2-n^2)$$

making $\beta = 0$ in these equations

$$\alpha^2 = m^2-n^2, \qquad \alpha = \pm\sqrt{(m^2-n^2)}$$

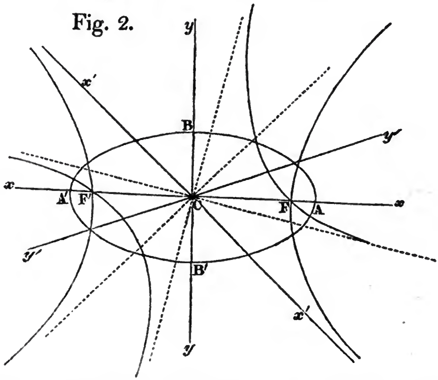
making $\alpha = 0$, $\qquad \beta = \pm\sqrt{(n^2-m^2)}$

If $m > n$, the latter value of β is imaginary, and therefore these curves cut one another in two points only, which are situated on the major axis at the distance $\pm\sqrt{m^2-n^2}$ from the centre. This result is entirely independent of the quantities a, b, a', b', that is, of the direction of the axes x' and y'; and hence it is evident that there are only two points from which the distance r to the curve is rational in terms of a'' and b''. These points are called the foci.

The

The curves whose intersections give the foci are equilateral hyperbolas. (See fig. 2.)

Fig. 2.



It is easy to show generally that the equation to the curve being

$$Ax^2 + Bxy + Cy^2 + Dx + Ey + F = 0,$$

the foci result from the intersection of the curves:

$$\{B^2-4AC\}(x^2-y^2)+\{2BE-4DC\}x-\{2BD-4EA\}y + E^2-4CF-D^2+4AF=0$$

$$\{B^2-4AC\}xy+\{BD-2AE\}x+\{BE-2DC\}y+2BF-ED -\big\{\{B^2-4AC\}y^2+\{2BD-4AE\}y+D^2-4AF\big\}\cos xy=0$$

VIII. *Particulars of the Measurement, by various Methods, of the instrumental Error of the Horizon-Sector described in* Phil. Mag. vol. lix. *By* John Nixon, *Esq.**

THE horizon-sector may be described, sufficiently for our present purpose, as a telescopic-level, having attached to each side of its telescope a vertical plate with a divided arc carrying at the centre of the divisions a short horizontal axis on which moves an index supporting a spirit-level. The Ys are fixed to a bar of brass, of which one end, formed into a sort of horizontal axis, connects the bar to a frame or stand beneath, which latter serves, by means of rack-work, &c. to raise or depress the telescope.

* Communicated by the Author: see Phil. Mag. vol. lix. p. 130.

First

First Method.—By finding the horizontal inclination of the upper surface of the telescopic tube, resting within its Ys, with the bubble of its level at the reversing point.

Support for the Sector.—A block of compact micaceous sandstone, (estimated to weigh two tons and a half,) hewn, with trifling waste of the material, into a pillar nine feet long, and of the average diameter of two feet, was set up, at the bottom of a pit four feet and a half deep, with its thicker end based on bedded rock. The excavation being gradually filled up with earth well rammed down, the pillar became so firmly fixed, that no change of situation of the observer, nor application of his utmost force against its sides, affected its inclination more than a fraction of a second. After a lapse of some days (extremely variable in the temperature), the top of the pillar was worked as even and as horizontal as practicable.

Three cylinders of lead, each three inches in diameter and nearly an inch thick, introduced within the lower part of the frame of the sector, served effectually to keep it steadily in its place upon the pillar.

Description of the Proof-Level.—The level tube, made to order by the celebrated Fortin of Paris, is twelve inches long, one inch in external diameter, but considerably less within, the sides (to prevent flexure) being so thick as to require some care in reading off the divisions without parallax. Each end of the tube is cemented into a brass socket or hollow cylinder one inch and a half in length. The scale contains 230 divisions, of one *millimetre* each, etched on the glass and stained red, each division being equal to little more than half a second, equivalent to one sixteenth of an inch for one second.

The frame of the level was a well-squared bar of mahogany full 17 inches long, 1·5 broad, and 2 inches deep (or high). At equal distances from the ends two horizontal brass plates $\frac{3}{4}$ of an inch long, $\frac{1}{3}$ of an inch thick, and nearly of the breadth of the bar were fastened to its upper surface by two strong screws. For the support to Fortin's level-tube each plate carried a Y (formed out of the same piece of brass as the plate) placed at a distance of 10 inches asunder, with their angular points in a line parallel to the (vertical) sides of the frame.

A brass plate $\frac{3}{4}$ of an inch long, $\frac{1}{3}$ of an inch thick, and of the breadth of the frame was secured to each end of its under surface by four long screws driven in nearly at the corners of, and level with* the plates. These plates (previously attached to the frame) were ground to a true plane upon two† contiguous iron

* In the measurements no part of the screw-heads would come in contact with the cylindrical rings of the sector.

† The diameter of neither plate being equal to the length of the bar.

iron (?) surfaces (used in grinding the receivers of air-pumps), fixed parallel to the horizon and level with each other by means of an adjusted spirit-level.

A few trials with the proof-level indicated that the cylindrical ring of the sector the nearest to the object-glass was slightly conical; for although the proof-level had but little play between the shoulders of the two rings, yet the nearer it was moved towards the eye-end shoulder the greater the deviation of the bubble in the opposite direction; the total range being about four seconds.

To obviate this cause of inaccuracy, a long smooth hone having an even surface was fixed upon a table, and at one end of the hone was secured, in a line with it, a board rising some little above the level of the hone. Hence by drawing the proof-level to and fro, and always in one direction, with one plate on the well-oiled hone* and the other on the board, the outer end of each plate was ground down at one and the same angle. To reduce the inward ends of the plates the hone was equally raised *above* the level of the board. The plates were now of the figure in the margin, so much only of the original surface being left as would come in contact with the cylindrical rings of the sector exactly over their Ys.

To each side of the frame, at an equal distance from the end of it, was attached an upright piece of wood, both pieces being notched above for the reception of a level-tube furnished with a divided scale of about $\frac{1}{40}$th of an inch to $2''$. This cross level, which served to set the brass plates *transversely* level, was adjusted in the following manner. A brass bar, 20 inches long, 4 inches broad, and $\frac{1}{2}$ an inch thick (including the rim), *professedly* ground to an even surface, was placed upon the stone pillar and secured at the corners by four heavy weights. The bar being of uniform thickness and the top of the pillar nearly horizontal, the proof-level could be placed with its ground plates in contact with the upper surface of the bar and reversed in direction without displacing the bubble of the cross level beyond the limits of its scale. Hence the plates would be horizontal in a *transverse* direction when the middle of the bubble stood at a point of the scale equal to the mean of the readings before and after reversing the frame.

The brass bar, although certainly not perfectly even, could

* To prevent the plates being *burred* in the operation the hone was constantly supplied with oil, and no other force applied than the weight of the bar.

not

not be more than 1′ or 2′ out of plane; a quantity too small to vitiate the measurements *.

Measurement of the equivalent angle of the divisions of the level scales.

1. *Sector levels.*—The sector was placed upon the pillar in the shade, weighted, and the bubble of the cross level brought to its mark. When the sector had acquired the temperature of the air, (noted by a thermometer placed close to the pillar,) the zero of the index carrying the level then uppermost was made coincident with that of its arc. With the nut of the *stand* the bubble was brought nearly to the beginning of the scale, (numbered onwards from the eye-piece towards the object-glass,) and by-and-by noted and registered for both ends in one column A. With the nut of the *index* the latter was moved until the bubble had run nearly to the object-end of the scale, and its ends, perfectly stationary, registered in another column B. This process forms one observation. Secondly, with the nut of the *stand* the bubble was again brought towards the eye-end, and by the nut of the *index* afterwards moved towards the object-end of the scale. This constituted the second observation. When five observations were completed, the total arc was read off without disturbing the instrument, and the process repeated up to about ten observations, when the final arc was read off. Half the difference between the sum of the column A and that of the column B is the degrees of the scale answering to the arc last read off, and dividing the latter by the above difference we obtain the value in seconds of one division of the scale. This value we can confirm by the arc first read off and the difference of the two columns up to the time of reading off. The results are subjoined.

Right-hand Index Level.

March 6th, 1830.	Arc 6′ 17″	1° = 1″·74	Temp. 36°·5
	7 0	1 ·76	36

Left-hand Index Level.

March ... 1830.	Arc 7′ 5″	1° = 1″·93	Temp. 43°
	8 0	1 ·91	— 40
April 14th, 1830.	10 38	1 ·96	— 50
May	8 23	1 ·89	— 68
	6 32	1 ·90	— 67

* Admitting the rings of the sector to form together the frustum of a cone of which the axis (during the measurements) is inclined 16″ and the upper surface 28″, the error (*minus*) introduced by an inaccuracy of adjustment of the cross level amounting to 10′, would be equal to (28−16) = 12″ × versed sine of 10′; or to 0″·00005.

In most of the measurements the range of the bubble was confined between points of their scales made use of in the subsequent operations, but the result was not materially different from that obtained by trials on nearly the whole extent of the scales.

Fortin's Level.—The value of its divisions was variously ascertained: 1st, Precisely as just described, the tube being laid upon the under surface of the horizontal bar of one index of the sector; to which it was attached, in one case by a string, and in another by a little glue.—It was fixed parallel to the telescope of the sector with the surface of the fluid within the tube equally bisected longitudinally by the line passing through the middle of its divisions. (When transferred to the proof-frame, (the cross-level of the latter being at its mark,) before the tube was fastened within its Ys, it was made to have the surface of the contained fluid bisected as just described.) Another method of finding the value of the divisions when the tube was attached to the proof-frame, was to place it upon the rings of the sector, and compare the run of its bubble at different variations of inclination of the sector, with that of the levels of the latter. This method is free from every objection to which the other is liable, except that of regulating a large instrument by a smaller and inferior one. The results were as follows:

March 5th, 1830. Fortin's level-tube attached by strings to index bar of sector.

Arc	3′ 7″·5	1° = 0″·60	Temp.	43°
	3 ·10	1° = 0 ·60	—	43

April 1830. The level-frame fastened at its ends, by ribbons, to the cylindrical rings of the sector, and compared with the right-hand level of the latter;

Arc............ 1° = 0″·65 Temp. 47°

April 1830. The frame glued to the inside of the vertical arches of the sector, and compared with the left-hand level of the latter;

Arc .... 1° = 0″·63 Temp. 52°

April 1830. The brass sockets of the level glued to the index bar of the sector, and a ribbon passed tightly over the middle of the glass tube and the index-bar. The run of the bubble amounted to 1721 divisions.

Arc 17′ 17″ 1° = 0″·60 Temp. 60°

April 1830. Repeated with the ribbon cut off, which caused the bubble to run immediately exactly 1″, from one end towards the middle of the tube. The result was unsatisfactory, from the

the great discordance between the values by the first and second arcs.

Arc $10' \, 29''$ $1^\circ = 0''\cdot65$ Temp. 60°.

April 1830. Same as last, but the final arc only was read off.

Arc $9' \, 33''$ $1^\circ = 0''\cdot65$ Temp. 60°.

The experiments on levels differently mounted (given in Phil. Mag. and Annals, vol. v.), led me to anticipate some of the above discordances. Probably they were increased by the weight of Fortin's level-tube, which might be sufficient, when resting upon the index-bar (which has no apparatus to clamp it to its arc), to depress it gradually. On this account it would have been advisable to have made two sets of observations, one with the telescope elevated, and the other with it depressed. In the former case the weight of the tube would tend to give an arc in excess, and in the latter case in defect.

(In reply to my inquiries, M. Fortin informed me that he had not ascertained the value of the scale with extreme exactness, but that the divisions would be about half-a-second *sexagesimal* each.)

Process of measuring the horizontal inclination of the cylinder.

There are three methods. One is, to place the upper surface of the (supposed) cylinder exactly parallel to the horizon by means of the proof-level, and then to take it out of its Ys and replace it within them reversed in direction; the consequent deviation of the bubble of the sector-level being double the horizontal inclination. Or, if we have previously determined the reversing point of the sector-level, the difference between that point and the one at which the bubble stands when the surface of the cylinder has been rendered horizontal by the proof-level, gives, without reversion of the cylinder, the inclination required. In this method it is not necessary that the proof-level should be strictly parallel to the cylinder, or its plates truly horizontal in the transverse direction; but, on the other hand, we measure the inclination by the sector-levels which are inferior to that of Fortin. As it is almost impossible to place the cylinder with its upper surface horizontal to the fraction of a second, it must be noted which end of the cylinder stood the highest, and the amount, in the case of the thicker end, must be deducted from the measurement.

Every visible particle of dust being carefully removed from the cylinder and the Ys, the proof-level was placed upon the cylinder with one side of the bar resting within and against the adjacent arc of the sector, and the opposite side of the bar against

against the other arc*. The cross-level of the sector being *exactly*, and that of the proof-level *nearly* at their respective marks, the latter was rendered perfectly so (a little at the expense of the sector cross-level) by moving the arcs a second or so out of a vertical plane. The proof-level and that of the sector being read off, the former was replaced reversed in direction, and read off, and finally, the cylinder being reversed within its Ys, the sector-level uppermost was read off, but not before the proof-level had been replaced, as its weight was found to alter the sector-level about 1″.—The process was then repeated, on the opposite surface of the cylinder, with the other level of the sector. In these observations it was always noted whether the telescope pointed north or south, and the readings for all the levels were registered in two columns, one for the north ends, and the other for the south ends of their bubbles.

The conical figure of the object-end ring of the sector has already been pointed out. With the reduced plates the proof-level could, however, be pushed close to the shoulder of either ring without causing a deviation of more than half-a-second in Fortin's level. In reversing the sector the effect of the conical ring would be even more insignificant; the play within the Ys being scarcely perceptible.

The measurements with the reduced plates were made, April 13th, 1830; Fortin's level tube being secured within the Ys of the frame by broad ribbon passed round the sockets of the tube and the frame, and made perfectly tight by stitching together the ends of the ribbon with strong thread.—On placing the eye in the plane of the horizontal plates of the proof-level, their contact with the surface of the cylinder was so uninterrupted, that scarcely a glimmering of light could be seen anywhere between. The temperature being 49°, the horizontal inclination appeared to be 31″·1 by the right-hand level, and 29″·2 by the other; mean 30″·15. The discrepancy may arise from that surface of the eye-end ring, which is uppermost when the right-hand level is made use of being slightly corroded; or partly because the scales of the two levels have not had their angular equivalents determined with equal accuracy.

Second Method.—The cylinder being placed within its Ys with the bubble of its level at the reversing point, the inclination is found by placing the proof-level upon it *direct* and *reversed;* half the run of the bubble giving the inclination required.

In this method, especially when the cylindrical error is con-

* The arcs are not exactly parallel to each other.

siderable,

siderable, some care is required in keeping the plates horizontal in the transverse direction, and in making the proof-level parallel to the cylinder; both sources of error tending to give a measurement in defect. In some instances it was attempted to place the proof-level parallel in direction to the cylinder, by bringing the middle of each end of the former to the highest point of the adjoining shoulder of the latter; the bar being prevented from toppling by a delicate wedge of mahogany gradually introduced between the sides and the arc until the cross-level of the frame arrived at its mark. When the proof-level was placed in contact with both arcs (which was more frequently the case), the deviation from parallelism with the cylinder could not possibly amount to a tenth of a degree,—a quantity too small to be sensible in its effect on the measurements*.

It does not appear that there were any trials of this method with the plates reduced, and the results of those made previously were, for the reason assigned, too discordant to be worth transcribing.

Third Method.—This differs from the preceding one solely in completing the measurements without reference to the levels of the sector; the proof-level being noted, both direct and reversed, when in contact with the cylinder *before* as well as *after* the latter is reversed within its Ys. If the angular points of the Ys are level with each other, the proof-level will give the inclination of the cylinder alike before and after the latter is reversed; otherwise, if the inclination be made too much *before*, it will come out equally in defect *after* reversing the cylinder†.

Fortin's level-tube being glued at the sockets to the Ys of the frame, the inclination of the cylinder given April 16th, 1830, temperature 58°, by the third method, was 26″·9 with the left-hand level upwards, and 29″·1 with the other level uppermost. On the same day, the measurements repeated at a temperature of 56° were respectively 27″·3 and 29″·2; the mean of the whole being 28″·1, or 2″ less than by the first method.

Multiplying the mean of the two methods by 0·586 (the Ys opening at an angle of 90°), we obtain 17″·1 for the constant error of collimation of the sector‡.

* When we measure the inclination of a plane, if we do not place the level-tube exactly at right angles to a level line drawn on the plane, we obtain a measurement (in defect) equal (when the deviation and inclination are both small) to the correct quantity divided by the *secant* of the angle of deviation.

† By precisely the same process may be found the inclination of the (mathematical) axis of rotation of a transit instrument having unequal pivots.

‡ See page 429 of last volume.

It

It is a good verification of the measurements to find the reversing points of the proof-level the same on the cylinder as on a plane made as nearly horizontal in every sense as possible. But it is to be remarked that unless the plates of the proof-level are true planes, the reversing point may not be the same on the cylinder as on the plane. For want of a perfect plane this satisfactory confirmation could not be obtained; but the reversing points with the proof-level on the cylinder, as may be seen from the statement below, agreed within the fraction of a second of their mean value.

April 13th, 1830. Temp. 50°. Reversing point		105°
		105
		107
		107
	Mean	106
April 16th, 1830. Temp. 58°		$113\frac{1}{4}$
		113
		$113\frac{1}{4}$
		$114\frac{1}{4}$
		$113\frac{1}{2}$
		115
		$113\frac{1}{2}$
	Mean	$113\frac{3}{4}$
April 16th, 1830. Temp. 56°		$115\frac{1}{4}$
		114
		114
		$114\frac{1}{4}$
		$112\frac{1}{2}$
		$112\frac{1}{2}$
		112
		$111\frac{3}{4}$
	Mean	$113\frac{1}{4}$

(One division is equal to about half a second.)

There is this serious objection to all the three methods, at least in their application to the sector,—that the inclination of the cylinder cannot be measured on that part of its surface which comes in contact with the Ys during the process of adjusting the instrument.

[To be continued.]

IX. *Notes*

IX. *Notes on some recent Improvements of the Steam-Engines in Cornwall. By* W. Jory Henwood, *F.G.S. Member of the Royal Geological Society of Cornwall.*

To Richard Taylor, Esq. F.S.A. F.L.S. &c.

Dear Sir,

THE same excuse which was offered by Mr. Farey for his long silence on the subject of my paper* may, perhaps, now serve for mine on that of his†. I shall not attempt a regular discussion of his papers, as the digressions and the many objects they embrace, would, I fear, be unsuitable to your pages, and tiresome to your readers. I purpose confining myself to the various alterations and improvements which have been made in this county since Messrs. Boulton and Watt, and their agents, ceased to superintend the steam-engines on our mines; with the addition of a few incidental observations on some of Mr. Farey's statements.

In my former communication to you on this subject, I said that "variation in the elasticity of the steam employed by no means affects the invention [my views would have been more correctly expressed, had I said *the merit or principle* of the invention] of expansive working;" and, notwithstanding Mr. Farey's indignant attempts, he has advanced nothing which bears the semblance of *proof* of the contrary.

Mr. Farey says‡, Mr. Watt "proposed in 1782 to work his engines by stopping the supply of steam when the piston had only moved one-fourth of its course." Will that gentleman particularize the engines in this county which *now* expand *more* than three-fourths of their stroke? "Mr. Watt's engines with such boilers" (which will not retain steam of more than $3\frac{1}{6}$ pounds per square inch above the atmosphere) "cannot be made to exert a competent power to drain deep mines, unless the supply of steam to the cylinder is continued until the piston has run through more than half its course‡." This I humbly apprehend will depend on the size of the engine, and the weight to be lifted. I hope I shall not be charged with "altering" this sentence so as to *make* it "very indefinite."

In 1801–2, Captain Trevithick erected a high-pressure engine of small size at Marazion, which was worked by steam of at least 30 pounds on the square inch above atmospheric pressure. In 1804, as Mr. Farey admits §, the same gentleman introduced his celebrated and valuable wrought-iron cylindri-

* Phil. Mag. and Annals, N.S. vol. vii. p. 323.

† Ibid. p. 421, and vol. viii. p. 305.

‡ Ibid. p. 309.

§ Ibid. p. 313.

cylindrical boilers*, now universally used in this county. To these, every one at all acquainted with the Cornish improvements ascribes a great part of the saving we have obtained. This will further appear from an extract from a valuable work edited by John Taylor, Esq. F.R.S. †: the monthly consumption of coal in Dolcoath mine was in

"1807, 5112	bushels, and the mine was	being sunk below the	160 fath. level under adit: adit 42 f. deep.
1808, 5184	———	———	170 fathom level.
1809, 5688	———	———	———
1810, 6840	———	———	180 fathom level.
1811, 6912	———	———	———
1812‡, 4752	———	———	190 fathom level.
1813, 4536	———	———	———
1814, 5413	———	———	———
1815, 5954	———	———	———
1816§, 5322	———	———	———
1817, 3102	———	———	———."

The alteration in the boilers, was the introduction of Capt. Trevithick's cylindrical boilers in place of the common *waggon* boilers, which had until then been there in use.

In 1811–12, Capt. Trevithick erected a single acting engine of 25-inches cylinder, working with but two valves (the exhausting valve being wanting) at Huel Prosper Mine in the parish of Gwithian, which of course had a cylindrical boiler, in which the pressure of steam was more than 40 pounds on the square inch above atmospheric pressure; and the engine was so loaded that it worked full seven-eighths of the stroke expansively. Mr. Woolf, as Mr. Farey states ||, came to reside in Cornwall about the year 1813, and his "first engines for pumping water from mines were set up by him in 1814, at Wheal Abraham and at Wheal Vor mines in Cornwall; they had each two cylinders."

If the use of high-pressure steam acting expansively were a part of Mr. Woolf's patent, to which he could establish a legal and exclusive right; why did he not prefer a claim on Capt. Trevithick, or the adventurers in Huel Prosper Mine, who were using an engine of that description before that gentleman's arrival in Cornwall? The answer is obvious. Mr. Taylor remarks ¶, "During the whole of this year (1814),

* Phil. Mag. and Annals, N.S. vol. i. p. 127. † Records of Mining, p. 163. ‡ Alteration in the boilers this year.

§ In this year the old engine was replaced by a new one;—of this more hereafter. || Phil. Mag. and Annals, vol. viii. p. 308.

¶ Records of Mining, p. 156.

Jeffery

Jeffery and Gribble's engine at Stray Park performed the best duty, being the first that reached 35 millions, and maintaining for twelve months an average rate of 32 millions. Woolf's engine at Wheal Abraham was first reported in October in this year, and performed 34 millions. Number of engines reported, 29; average duty, 20,534,232." In the early part of 1816, a *single* engine of 76-inch cylinder was erected at Dolcoath mine by Messrs. Jeffree and Gribble (see preceding note), in place of an old and defective *double* one; which accounts for the diminished consumption of coal which the extract from Mr. Taylor's notice of Dolcoath exhibits. In the same year Mr. Woolf replaced an old engine of 63-inch cylinder, but which had for a short time previously been worked by higher steam than usual, with one of 66-inch cylinder at Huel Abraham: the latter, if I am not mistaken, was set to work about the 12th of November.

I believe I have now satisfactorily shown that Mr. Woolf, instead of being the *first* to introduce the expansive action of steam in one cylinder, was *positively preceded* several years by Capt. Trevithick, and *probably* so, a short time, by Messrs. Jeffree and Gribble. Mr. Farey says, "He" (Mr. Woolf) "altered another old engine at Wheal Unity, by adding a small cylinder to it; the performance was improved in about the same degree as that of the old engine with one cylinder *." Perhaps Mr. Farey may not be aware that after being altered the Huel Unity engine did not answer expectation, and the nossels which Mr. Woolf had put in were thrown out and replaced by others; this only made the matter worse, for the engine would not then keep the water ("*in fork*"); to remedy the defect these were removed, and those first put in were replaced. But, to mend the matter, for several years before the engine was stopped, the use of the small cylinder was dispensed with, and the engine worked as a common Watt's single engine; in which mode it is at present worked on Oppie's shaft in Poldice mine.

Whilst on the subject of alterations, I may remark that Mr. Woolf had intended adding a small cylinder to the old engine (48-inch cylinder, single) at Huel Vor but the untoward result of his experiment at Huel Unity, prevented his making the attempt, although a cylinder was purchased for the purpose. This was subsequently, after much labour in enlarging the apertures, used by Messrs. Jeffree and Gribble as cylinder for a small double acting rotatory engine erected to move a *stamping* apparatus on the same mine, where it is

* Phil. Mag. and Annals, N.S. vol. viii. p. 312.

still at work. Mr. Farey continues *, "In a short time after Mr. Woolf's patent expired, most of the old Boulton and Watt's engines in Cornwall were altered to work by high-pressure steam on his system: some few had an extra cylinder added." Instead of the pressure of steam in general use being raised at that period, I assert no such alteration took place in a more marked degree, then, than had before obtained; and the alteration which at all took place was not in consequence of Mr. Woolf's assertions or performances, but of those of Capt. Trevithick. Will Mr. Farey name an engine to which a cylinder was added (that it might be worked on what he calls Mr. Woolf's system) after the expiration of that patent? He proceeds †, "The advantage of the change from low-pressure to high-pressure steam, on Mr. Woolf's system, was manifest in all cases; but it was greater or less, according as the steam was used stronger and with more or less expansive action." Now, I maintain that there is not a shadow of ground for this assertion, and I challenge Mr. Farey to prove its accuracy. He says ‡, "Previous to 1826 the steam cases were not clothed, but exposed to the air." That the steam cases of Boulton and Watt's engines were covered with lath and plaster whited on the outside, is notorious; their steam pipes were also encircled with straw ropes plastered and whited. The steam case at Dolcoath engine was surrounded with a casing of sheet-iron; the interval of about four inches being filled with straw, hemp, saw-dust, and other imperfect conductors of heat. Mr. Farey says §, "Mr. Hornblower, who practised that system" (expansive working) "in two cylinders, did not succeed so well as Mr. Watt himself, who only used one cylinder." I repeat, that "variation of the elasticity of steam employed by no means affects the *principle* of the invention;" the superiority of one cylinder over two being proved with low-pressure steam, it appears to me "ignorance in spite of experience" to expect a contradictory result by variation of tension only; and, still stumbling over the results of his own experiments as well as those of Messrs. Watt and Hornblower, Mr. Woolf's engine with two cylinders at Huel Alfred was a signal failure.

The improvements which came prominently into notice in 1827 were commenced by Capt. Grose, at Huel Hope mine in Gwinear parish, in 1825. They do not in any part consist, as Mr. Farey states ‖, in "using better boilers;" for they are

* Phil. Mag. and Annals, N.S. vol. viii. p. 312. † Ibid.
‡ Ibid. p. 312–313. § Ibid. p. 312, note. ‖ Ibid. 313.

precisely

precisely similar to those which he before says* were "first brought into use for high pressure steam by Mr. Trevithick in 1804;" but in different arrangement of the flues† round them; first introduced at Huel Hope, afterwards at Huel Towan, and subsequently in many other mines (the Consolidated, Huel Vor, &c.); in some degree by more attention to the temperature of the hot well; and in a great proportion by carefully covering all the vessels containing dense steam with bodies which transmit heat very slowly.

That these originated with Capt. Grose, and were borrowed from him (without acknowledgement) by Mr. Woolf and others, will appear from the following extracts from Messrs. Lean's monthly reports.

1827.	Huel Towan Mine. Wilson's Engine‡. Grose, Engineer.	Consolidated Mines. Best Engine.§ Woolf, Engineer.		Huel Vor Mine. Trelawny's Engine. Sims and Richards, Engineers.‖
Jan.	48·9 millions.	Taylor's	42·8 millions.	43·5 millions.
Feb.	51·8 ——	Ditto	48·5 ——	43·1 ——
March	53·5 ——	Huel Fortune	41·9 ——	45·2 ——
April	61·8 ——	Ditto	44·1 ——	47·1 ——
May	60·6 ——	Ditto	43·9 ——	46·9 ——
June	61·7 ——	Pearce's	42·7 ——	49·1 ——
July	62·2 ——	Huel Fortune	40·8 ——	45·5 ——
Aug.	61·7 ——	Pearce's	43·2 ——	50·8 ——
Sept.	60·1 ——	Huel Fortune	42·5 ——	50· ——
Oct.	61·3 ——	Woolf's¶	63·7 ——	47·8 ——
Nov.	56·1 ——	Ditto	67· ——	50· ——
Dec.	57·7 ——	Ditto	62·4**——	48·4 ——

	Millions.
1827: average of all the pumping engines reported	31·9
—— Huel Towan, Wilson's engine......	58·1
—— Consolidated Mines, 12 months' average of best engine	48·6
—— Woolf's engine¶	64·4
—— Huel Vor Trelawny's engine.........	47·2

* Phil. Mag. and Annals, N.S. vol. viii. p. 313, note.
† Brewster's Journal, vol. ix. p. 159.
‡ Wilson's engine was at that time the best on Huel Towan.
§ In the first nine months; the same engine was not every month the best.
‖ Trelawny's engine is the best on Huel Vor.
¶ Woolf's engine was not reported until October 1827.
** At an experiment made by some Cornish engineers in this month the duty was 63·6 millions.

1828.

1828.	Huel Towan Mine. Wilson's Engine. Grose, Engineer.	Consolidated Mines. Woolf's Engine. Woolf, Engineer.*	Huel Vor Mine. Trelawny's Engine. Sims and Richards, Engineers.
Jan.	64·4 millions.	59·2 millions.	45·8 millions.
Feb.	73· ——	62·9 ——	47·9 ——
March	84·2 ——	62·3 ——	51·5 ——
April	87· ——	57·2 ——	55·5 ——
May	not reported.†	67·5 ——	60· ——
June	76·1 ——	61·5 ——	57·5 ——
July	75·8 ——	65·4 ——	60·2 ——
Aug.	81·9 ——	61·9 ——	not reported.
Sept.	81·6 ——	62·8 ——	54· ——
Oct.	77·8 ——	65·7 ——	55·7 ——
Nov.	74·2 ——	60·8 ——	61·1 ——
Dec.	73·6 ——	63· ——	55·7 ——

		Millions.
1828:	average of all the pumping engines reported	37·2
——	Huel Towan, Wilson's engine...	77·2
——	Consolid. Mines, Woolf's engine	62·5
——	Huel Vor, Trelawny's engine......	55·

I think the foregoing details prove, not only that Captain Grose led the van of the more recent improvements, but that he has kept far in advance of its strides. The importance of the saving thus obtained cannot be more plainly shown than by the following extract from Mr. Taylor's valuable "Records"‡. "In 1825, all the drainage of the Consolidated Mines was effected by three engines only; they were hard pressed by the increased depth of the mine and quantity of water, and derangement happened, which, added to the bad state of some boilers, and the pit-work, which suffered from the engines being unavoidably worked too fast, the duty of the engines fell off considerably, and, as the reports will show, did not average quite 30 millions; at that time the monthly consumption was

Job's engine	(90-inch cylinder)	4992 bushels.
Pearce's do.	(58 do.)	3615 ——
Bawden's do.	(90 do.)	8427 ——
		17034

"At the present time (1829) six engines are at work, as, to remedy the evils above stated, and to provide for sinking deeper

* For all 1828, it took the lead of the Consols'. engines.

† In this month an experiment was made by Cornish engineers, duty 87·2 millions; and in April 1830 by Mr. Rennie and myself, duty 92,260,202 by calculation, and by *water delivered* as determined by a float 83,602,022.

‡ Records of Mining, p. 164.

deeper, three additional ones have been erected, and the consumption of fuel now stands as under, the mines being 20 fathoms deeper than in 1825:

Job's engine	(90-inch	cylinder)	about	2050	bushels.
Pearce's do.	(65	do.	)(altered)	2130	——
Bawden's do.	(90	do.	) about	2050	——
Taylor's do.	(70	do.	)	1480	——
Woolf's do.	(90	do.	)	1710	——
Shears's do.	(70	do.	)	1180	——
				10,600	

"The average duty of the six engines is now reported at more than 50 millions." This difference I attribute entirely to Capt. Grose's improvements. Mr. Farey has thought fit to charge me* with keeping back the fact of our engines having only one cylinder, and being worked with high-pressure steam, acting expansively. If he will refer to Brewster's Journal, vol. ix. p. 160, and vol. x. p. 42, he will find papers (which although inaccurate in some minor details are substantially correct) in which the elasticity of the steam employed is particularly mentioned. He has also said that I have, in quoting, "*altered*" his expressions, so as to make them "*very indefinite*"†. I believe my quotations present his ideas in quite as *definite* a form as that in which they stand in the original; and I do not base this assertion on my own opinion only.

I may now be asked, in what respect Mr. Woolf has been a benefactor to Cornwall? I reply, In introducing accurate workmanship and some attention to proportion in the construction of engines, both which disappeared on Messrs. Boulton and Watt's removal. These I take to be the only benefits he has conferred on the county; but they are favours of no mean order, as those who know the state of the engines in Cornwall, before his arrival in the county, will readily admit.

I pass over some points of minor importance on which I could have desired to make a remark or two, as I fear I have already trespassed too long on your patience, and I am half ashamed that the discussion has assumed so unconnected a form; but however much I may be disposed to enter further upon it, I shall deny myself the honour, unless the inquiry be in future directed to but one point at a time. Yours, &c.

Perran Wharf, near Truro, April 11, 1831. — Wm. J. Henwood.

* Phil. Mag. and Annals, N.S. vol. vii. p. 423. † Ibid. p. 422.

X. *On*

X. *On Chemical Symbols and Notation; in Reply to Professor* WHEWELL. *By Mr.* JOHN PRIDEAUX, *Member of the Plymouth Institution.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN the last Number (May 1831) of the Journal of the Royal Institution, is an article by Professor Whewell, "On the employment of Notation in Chemistry;" going to show the advantages of using symbols in that science; and to remove anomalies in the foreign notation, by reducing it to mathematical consistency and symmetry. I concur with all that is therein stated of the practical convenience and utility of such symbols, and I am glad to see Professor Whewell's eminent name enlisted in their advocacy. They constitute a prompt, impressive and peculiarly legible short-hand; expressing in a few letters and dots what would occupy several lines of writing, and what no writing will give with the same distinctness,—a great point in a science of which the materials and modifications are so numerous and so complex. In experimental notes they are of great service, placing constantly before our eyes the entire analysis of every material employed; and for the same reason, as well as on account of their compactness, are aptly suited for comparison and tabular classification. But I am not yet convinced that it is desirable to sacrifice this graphic simplicity, in order to convert them into an algebraic notation; to which, when requisite, they are easily accommodated. Such of your readers as feel interested in this question, and have not seen Professor Whewell's paper, would do well to read it before proceeding with this.

The chemical symbols of Berzelius were contrived "pour faciliter l'expression des proportions chimiques, et de nous mettre en état d'enoncer brièvement et avec facilité le nombre d'atômes élémentaires qui se trouve dans chaque corps composé." To this purpose they seem happily fitted; exhibiting, in signs no less natural than compact, the entire constitution of the substance through all the grades of analysis.

Thus an atom of potassium, K (*kalium* in Berzelius's Latin), oxidized with one atom oxygen $\dot{K}$, forms potash; an atom of nitrogen N, with five atoms oxygen $\overset{\cdot\cdot\cdot\cdot\cdot}{N}$, forms nitric acid, and $\dot{K}\overset{\cdot\cdot\cdot\cdot\cdot}{N}$ forms nitrate of potash.

In this concise symbol we see the acid and alkali, and the elementary composition of each: the bases expressed in their initial letters; and oxygen, the great modifier of chemical properties;

properties,—the pivot, as it were, round which chemical agency revolves,—is exhibited, in a manner equally characteristic, prominent and compact, by dots, corresponding to the number of atoms placed over the symbol.

Again, H is hydrogen; H^3 three hydrogen; and NH^3 ammonia. C is carbon, C^2 two carbon; and C^2N cyanogen. HC^2N is hydrocyanic acid; and NH^3HC^2N hydrocyanate of ammonia.

Al is aluminum; $\dot{\mathrm{Al}}$ alumina; S sulphur; $\dddot{\mathrm{S}}$ sulphuric acid; and $\dot{\mathrm{Al}}\,\dddot{\mathrm{S}}$ sulphate of alumina. A compound salt is expressed by joining its proximate ingredients through the sign +; thus $\dot{\mathrm{Al}}\,\dddot{\mathrm{S}}+\mathrm{NH}^3\dddot{\mathrm{S}}$ is sulphate of alumina and ammonia. When one of the constituents is in more than one atomic proportion, it is preceded by the indicatory multiple: thus $\mathrm{NH}^3\dddot{\mathrm{S}}+3\,\dot{\mathrm{Al}}\,\dddot{\mathrm{S}}$ is ammoniacal alum, which crystallized with 25 atoms water becomes $\mathrm{NH}^3\,\dddot{\mathrm{S}}+3\,\dot{\mathrm{Al}}\,\dddot{\mathrm{S}}+\mathrm{Aq}^{25}$.

(It is my custom to write the salifiable bases in inclined, and the acids in erect letters, $\mathit{NH}^3\dddot{\mathrm{S}}+3\mathit{Al}\,\dddot{\mathrm{S}}+\mathit{Aq}^{25}$, which is attended with some conveniences; but has not received the sanction of Berzelius, and might, though in my practice it has not so occurred, *possibly* interfere with his mineralogical symbols.)

A principal objection, in the paper above quoted, is, that the symbols are joined in the way which, in algebra, denotes multiplication.

But this juxta-position $\dot{\mathrm{K}}\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{N}}$, and the employment of an index figure, NH^3, as practised by Berzelius, lead the chemist into no error, because their subjects are not susceptible of algebraic *powers*, or of being multiplied into each other. We can have neither cube nor square of H (hydrogen), nor obtain a multiple of $\dot{\mathrm{K}}$ by $\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{N}}$: and the cases in which atomic calculation would necessarily generate such products are so rare that the letters x, y, &c. may be substituted, or the actual symbols adapted to the occasion; without general destruction of their conciseness, by multiplication of signs, brackets and letters. Chemical mathematics are mostly simple, but a difference is to be made in the symbols, according to the more or less compound character of the individual subjects of calculation. If simple bodies were concerned in the process, we should represent them as o, oxygen; c, carbon, &c. If alkalies and acids, as potash, nitric acid, &c. we should state them l, n; or C, n', if preferred. But with respect to tri-silicate

of alumina, sulphuret of iron, arseniuret of cobalt, &c., we should employ a single letter for each, unless their decomposition were concerned in the research. In short, however contrived, they must be variable and varied, as in other sciences, for algebraic adaptation; and they would be analytical so far only, as decomposition is connected with the calculation, as will be exemplified below.

Although juxta-position implies multiplication in algebra, the signification is arbitrary; and in another science, equally mathematical, with elements susceptible of every mathematical power, we can so place them without fear of understanding CLV as C × L × V, or 534 as 5 × 3 × 4. This position implies naturally (if the word be allowable) the meaning Berzelius attaches to it, *alliance* or *combination;* the truth of which is illustrated in the paper under review; in which one of the ablest algebraists of the age, in the very act of publicly arraigning it, writes

Protoxide of manganese	$mn + 0$	M n
Sesquioxide...............	$mn + \frac{3}{2}0$	M $n\,s$
Deutoxide................	$mn + 2\,0$	M $n\,n$

and by the same rule would write the oxides of osmium,

$$O\,m, \quad O\,m\,m, \quad O\,m\,m\,m\,m.$$

Two cases are quoted from Herschel, to exemplify the advantages of a more mathematical system. One describing "the decomposition of oxynitrate of silver by hyposulphite of lime."

"L represents lime; S, sulphur; *s*, silver; O, oxygen; N, nitric acid. He says, 'we have for the atoms present, before the decomposition,

$$\{L + 2\,(S + O)\} + \{N + (s + O)\}$$

which afterwards group themselves thus:

$$= (L + N) + (S + s) + (S + 3\,O)$$

that is, one atom of nitrate of lime, one of sulphuret of silver, and one of free sulphuric acid.'"

The other is of kindred character; and they are given as "very instructive examples of the use which may be made of such a notation." The system of Berzelius is said to possess no such advantages.

Of the case quoted it may be observed, that the sign + alternates with every letter, whether indicative of intimate combination, mixture or decomposition; and that an incompatible mixture is made = equivalent to a compatible one*:—that

* The sign = in Mr. Whewell's paper is employed only in the second example; but the circumstances being the same, it is needless to quote both.

the

the symbols are severally constructed for the occasion, free from the restrictions of any rule of intelligibility; lime, composed of an atom of calcium and one of oxygen, being signified by L; whilst oxide of silver, also of an atom of each ingredient, is made $(s + O)$: sulphuric acid, one atom sulphur and three oxygen, is $(S + 3\,O)$; whilst nitric acid, of five atoms oxygen to one nitrogen, is only N. The chemistry is in fact sunk in the mathematics, simple as they are; and the scheme, without the text, is totally unintelligible.

Berzelius would express it thus: $\dot{Ag}\,\overset{\cdot\cdot\cdot\cdot\cdot}{N}$ with $\dot{Ca}\,\dot{S}^3$

gives $\dot{Ca}\,\overset{\cdot\cdot\cdot\cdot\cdot}{N}$, AgS and $\dddot{S}$

$\dot{Ca}$ being lime; $\dot{Ag}$, oxide of silver; $\dot{S}^2$, two atoms hyposulphurous acid, &c.

Here you have the whole process (its chemical probability being not in question) in the symbols, as well as the proximate and ultimate analyses of each ingredient.

We may now contrast the proposed notation with the examples before given of the symbols.

	Berzelius.	Whewell.
Nitrate of potash	$\dot{K}\,\overset{\cdot\cdot\cdot\cdot\cdot}{N}$	$(k+o)+(n+5\,o)$
Hydrocyanate of ammonia	NH^3HC^2N	$(n+3\,h)+(\overline{n+2c}+h)$
Ammoniacal alum	$3\dot{A}l\,\dddot{S}+NH^3\dddot{S}$	$\{\overline{3(al+o}+\overline{s+3\,o)}\}\overline{+(n+3h}+\overline{s+3o)}$

	Do. contracted.
Nitrate of potash	$K+n'$
Hydrocyanate of ammonia	$Am+h\,c'\,n$
Ammoniacal alum................	$(3\,\overline{Al+s'})+(A\,m+s')$

The multiplication of lines and brackets in the proposed notation gives it, in my view, a comparatively perplexed appearance, though algebraically just. The unvarying sign + alternating with every letter, loses the distinctive property, in which consists its value. The increased length of the formula destroys its graphic character: and in the contracted notation this graphic property and the analytical expression are both incomplete.

To apply this notation in the case quoted from Herschel,

$$\{(c + o) + 2\,(s + o)\} + \{(n + 5\,o) + (ag + o)\}$$
$$= \{(c + o) + (n + 5\,o)\} + (s + ag) + (s + 3\,o).$$

or, contracted,
$$(C + 2\,s') + (Ag + n')$$
$$= (C + n') + (Ag + s) + s'.$$

Here the full notation appears confused by the needless

 multiplicity

multiplicity of terms, &c.: the contracted does not apply; but both together will do very well. This simple case, then, amply illustrates the need of varying the structure of our symbols according to the occasion, when employed mathematically.

The chief objection made to the *mineralogical* signs of Berzelius is, that "the analysis itself is not recorded" by them. But here would seem to be some misunderstanding. Their difference from his chemical symbols is, that they do express the result of analysis *only*, apart from atomic relations; the letters signifying merely the names; and the figures giving the relative quantities of the common ingredient, oxygen; and, by consequence, the constituents, from the known proportions in which they contain it. Thus apophyllite is given by Berzelius, (*Nouveau Système de Minéralogie*, p. 225,) $KSi^6 + 8\,CSi^3 + 16\,Aq$: recording, that the oxygen in the lime is 8 times; in the silica, $6 + 8 \times 3 = 30$ times; and in the water, 16 times that in the potash: whence the respective ingredients may be read off, on the scale of equivalents, $\dot{K}$ 5·95; $\dot{C}a$, 28·25; $\dot{S}i$, 60; Aq, 18. And the cases quoted at p. 449 of Professor Whewell's paper are rather *equivalent* than *identical*.

Berzelius employs these signs in mineralogy, because "quant aux formules chimiques, elles sont sujettes à changer d'après des changemens dans nos idées du nombre des âtomes élémentaires dont chaque substance est composé." *Nouv. Syst.* 190. This is exemplified in the change of some of his own views since that treatise was written. He would then have construed the above formula, chemically, thus: $\ddot{K}\,\dddot{Si}^4 + 8\,\ddot{C}a\,\dddot{Si}^2 + 32\,Aq$. *Now* he regards potash and lime as each containing a single atom of oxygen; and the chemical formula would be $\dot{K}\,\dddot{Si}^2 + 8\,\dot{C}a\,\dddot{Si} + 16\,Aq$; or without grouping $\dot{K} + 8\,\dot{C}a + 10\,\dddot{Si} + 16\,Aq$; the proportions being still preserved. And hence these signs seem peculiarly adapted to the purpose of mineralogical record.

The mineralogical notation proposed in the article quoted, would require a local and chronological date to every recorded analysis, or would involve them in the hypothetical changes of atomic chemistry. Had the above analysis of apophyllite been so stated, the quantities of lime and potash would have been reduced one half by the publication of Berzelius's last atomic table; and the silica would have been reduced two thirds by the formula being reprinted in England.

With respect to the grouping in these formulæ, the views of

of Professor Whewell appear to me to be just. The crystalline form must be affected by the state of the combination; and arbitrary or hypothetical statements may impede our attainment of correct views on a subject of prime importance in mineralogy.

But I cannot yet see the propriety of sacrificing the concise and graphic perspicuity of the Berzelian symbols, for the purpose of reducing them to a notation, perhaps not very much more generally applicable to mathematical use; far less so to that of temporary record and tabular comparison in operative chemistry; and in mineralogy of hazardous admissibility.

Yours, &c.

Plymouth, June 4, 1831. J. PRIDEAUX.

XI. *On* Poonahlite, *a new Species of Mineral; on the Identity of* Zeagonite *and* Phillipsite, *&c.; and other Mineralogical Notices. By* H. J. BROOKE, *Esq. F.R.S. L.S. & G.S.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I SEND you herewith a few mineralogical notices, which will probably interest some of your readers.

I am yours, &c.

June 11, 1831. H. J. BROOKE.

Thulite.

In a list of minerals which I published in 1823, as an Appendix to an elementary work on Crystallography, I described this mineral as having a cleavage parallel to the planes of a rhombic prism of 92° 30′, which description was given on the authority of some fragments of a reddish mineral received by Mr. Heuland, from Sweden, under the name of *Thulite.* I since find that the mineral I then measured was *Bisilicate of manganese.* I have lately obtained and measured the true *Thulite*, and have found it agree in its cleavages and angular measurements with *Epidote*, as it had been before found to do by Mr. Levy.

Zeagonite.

In the same list of minerals I described the crystalline form of *Zeagonite* as an octahedron, with a square base; and I did this upon the authority of a crystal from Vesuvius, so named in the ticket by which it was accompanied. The crystal I then measured has since been called *Zircon*, but whether analysed or not I do not know. I have lately obtained specimens of the *Zeagonite* described by Gismondi, which I shall probably make

make the subject of a distinct communication, having satisfactorily ascertained that *Zeagonite*, *Abrazite*, *Aricite* and *Phillipsite* are one and the same mineral.

Velvet Copper Ore of Werner.

On dissolving this mineral in dilute nitric acid, a skeleton remains which is insoluble in any acid; and when the very minute portion I had to examine was placed on charcoal before the blowpipe with a drop of nitrate of cobalt, it ultimately became black; whence I concluded it to be silica. The part dissolved in the dilute acid contained sulphuric acid, copper and zinc.

Native Nickel,

so called, although a sulphuret. I have measured the fibres of this substance, and find them regular hexagonal prisms with apparent cleavages oblique to the axis, but the cleavage planes are too imperfect for accurate measurement.

Poonahlite.

I am indebted to the kindness of Mr. Heuland for some specimens of a beautiful variety of apophyllite from Poonah, in the East Indies, accompanied by some slender crystals, which I at first supposed were mesotype or needle-stone, but which differ from both substances in measurement; the *Poonahlite* being a rhombic prism of 92° 20′. The crystals traverse the mass of the apophyllite and matrix instead of forming groups in the cavities; and among several hundred crystals which I have examined on my own and on Mr. Heuland's larger specimens, I have not observed one with a natural termination. The hardness is nearly the same as that of needle-stone, as far as I can discover from an experiment on very small crystals.

Glaucolite.

This mineral has a cleavage parallel to the planes of a rhombic prism of 143° 30′ nearly.

Couzeranite.

This is described in Leonhard's Handbuch, as a *right rectangular prism*, and by Dufresnoy in the *Ann. de Chim. et de Phys.* xxxviii. p. 280. as an *oblique rhombic prism;* and it would appear from the analysis of the latter to be a distinct species of mineral. Mr. Heuland has lately supplied me with a specimen containing this substance in small imbedded crystals; on examination of which I find that it has the form, cleavage, and angular measurements of felspar. The crystals are small, and the matrix in which they are imbedded is partly white and partly black. Those in the white part are colourless and translucent;

translucent; those in the black part are black and opaque, and probably coloured by the same kind of matter as colours the matrix. Hence the analysis of the black crystals, which are the only ones yet analysed, must fail to give the true constituents of the mineral, and the theoretical chemical formula must be incorrect. The crystals are similar in form to the small single crystals of felspar coated with chlorite, which are brought from St. Gothard.

Pseudomorphous Crystals from a Mine at Haytor, in Devonshire.

In the year 1827 some crystals were found in this mine, which were described by Mr. W. Phillips and Mr. Levy, under the name of Haytorite*. It was obvious that the substance of the crystals was calcedony; and as they nearly agreed in form and angular measurement with Humboldtite, it was supposed they might have derived their pseudomorphous character from crystals of that substance, although from the solidity and state of aggregation of some of the crystals which were first discovered, it was difficult to imagine the mode by which the borrowed forms could have been produced. It is, however, equally difficult to comprehend the manner in which the well-known pseudomorphous crystals of steatite, imbedded in the same substance, could have been produced. These present the forms of quartz and of carbonate of lime, and they consist of steatite apparently homogeneous with the mass in which they are imbedded. We might indeed suppose that a cavity partly occupied by crystals of quartz and carbonate of lime had been filled by steatite; that subsequently the quartz and carbonate of lime had disappeared and left moulds which were afterwards filled by the *same kind* of steatite. But it is not easy to conjecture how the mould and the casts formed at very different times should be homogeneous. With regard, however, to Haytorite, there cannot now exist a doubt of its pseudomorphous character; many of its crystals being hollow, sometimes very thin, and the inner surfaces mammillated like ordinary calcedony. I have also a crystal of this substance which presents a form analogous to the common hemitrope crystals of sphene from St. Gothard, with a deep re-entering angle, and evidently formed within a polished cavity which it has only partially filled. But in addition to the evidences of pseudomorphism presented by many of the crystals of Haytorite, there have been found in the same mine other pseudomorphous crystals representing several of the forms of carbonate of lime, some of which are solid, and some hollow: among these are

* See Phil. Mag. and Annals, N.S. vol. i. p. 38, 40, 43.—Edit.

groups of very obtuse rhomboids, masses resembling pearl-spar, acute scalene dodecahedrons, and six-sided prisms with flat summits, or terminated by planes of the equiaxe crystal of Haüy; and in one instance that lies before me, the calcedonic cast represents one of those crystals of carbonate of lime which are frequently observed, in which a change from a flat to a modified termination of the hexagonal prism has only partially taken place.

XII. *On the Theories of Achromatisation, &c. in reply to* Dr. Goring. *By* The Rev. H. Coddington.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

MY name having been brought before the public by Dr. Goring, in an article contained in the last number of the Edinburgh Journal of Science, in a manner which, though apparently complimentary, must lead readers to consider me as a framer of apparently fine theories, which answer no good purpose in practice, a character for which those who know me are well aware that I entertain very little respect, I would request leave to make a few observations in reply, through the medium of your Journal, which I presume must find its way wherever any scientific periodical work is taken in.

Dr. Goring states that my work on the Reflexion and Refraction of Light, whether considered as a work of originality, or as a compilation from the writings of our first opticians, is admitted to be the best publication of the kind at present extant. So far I am obliged to him: but in the next article he "ventures, though well aware of what he is doing, to impugn the infallibility of exact science in the case of the theories of achromatisation of Professor Robison, Professor Airy, and Mr. Coddington, on the ground that no artist is able to make an achromatic instrument according to them."

Now, Sir, the plain truth of the matter is, that my work contains, to the best of my knowledge, exactly those theories which have guided all artists, from the elder Dollond downwards, who have been competent to refer to a theory at all. To these I have added one other, (to which Dr. Goring alludes in a note,) out of respect to its inventor Professor Airy. It is one of singular ingenuity and beauty, and gave every promise of a good result, but is probably imperfect, like many other speculations of the present day, since it certainly does not answer the end proposed. The author is well aware of this, and

and speaks of it, with his accustomed modesty, as of a youthful production, which has at least done no harm, except, indeed, as it appears, exhausting Mr. Tully's patience.

Dr. Goring, confessing that he cannot discover the least flaw in our theories, proposes to oppose them by facts, a practice which I cannot too highly applaud, being most firmly convinced that no theory can be considered as worth a farthing, except for the indirect advantage of exercising the intellect, until experience has shown that it is *complete* and *satisfactory*. Instead, however, of keeping this promise he proceeds to lay down certain "propositions, which rest only on the basis of the evidence of the eyes or experience." Some of these propositions unfortunately my own limited experience enables me most flatly to deny. I will beg leave to quote them in order, with a few remarks.

1. "When achromatism is obtained by the adjustment of lenses to particular intervals, as in the case of the Huygenian eye-piece, such achromatism is *absolute* and *perfect*, and not like that effected by the combination of a concave lens of flint-glass with a convex of plate or crown glass, which never effects a complete neutralization of the chromatic aberration, as is well known."

The Doctor seems here to confound together two distinct modifications of chromatic dispersion, which affect such instruments as a telescope or a microscope quite independently, and must be corrected, if necessary, on quite different principles, as I have endeavoured to explain, after Professors Robison, and Airy. The achromatism is in neither case perfect, and it is impossible to compare them, because they depend, though in different manners, on the apertures of the lenses employed as object-glasses and eye-pieces respectively, which are hardly connected with each other by any laws whatever.

2. "The only kind of achromatism, produced by convex lenses, which is known in practice, is when two are adjusted to an interval equal to one half the sum of their focal distance or thereabouts."

Every person who knows anything of the construction of a telescope is well aware that an *erecting eye-piece*, consisting of four lenses, is, when properly made, just as achromatic as the Huygenian.

3. "Many modifications of this combination may be made, as by doubling or tripling the eye- and field-glasses."

This is very true, and I have applied it with complete success, in order to correct a defect totally unconnected with chromatic dispersion.

4. "In

4. "In order to form a truly achromatic erecting eye-piece, there must be a compensation both in that part which erects or forms the image, and in that which views it; therefore *no achromatic erecting eye-piece can be made with so few as three lenses.*"

This is altogether incorrect both in theory and in practice. In an erecting eye-piece the compensation of dispersion may take place any how, provided only that it be completely effected on the whole. Erecting eye-pieces of three lenses are not used, though they may be made perfectly achromatic, because they are liable to another defect which cannot be compensated.

5. "An erecting eye-piece can only be made really achromatic, (if we do not employ concaves of flint glass,) by combining two Huygenian eye-pieces."

Did this gentleman ever examine the day eye-piece of a common hand telescope?

6. "Such an eye-piece could only be used for viewing an image, and could never be employed as an engyscope, because it would have no external focus in front of the bottom glass."

This is like No. 4, inaccurate, both as to the fact and the reason. Any erecting eye-piece may be used as a microscope, though it would serve little purpose on account of the very small magnifying power that can be thus obtained. Nobody would, however, find fault with it on that account, any more than he would condemn a good carving-knife, because it would not shave him clean. The Doctor then observes, that I have been at the pains of falsifying my own theories, *practically at least*, to the best of my abilities, by presenting to opticians a compound microscope, termed achromatic, which is constructed according to them. He says, "I assert point blank, that his instrument is as complete a failure as anything of the sort I ever attempted myself. I have examined one of these instruments of the latest and most improved construction, made by Mr. Cary, and can, I think, be positive that both the chromatic and spherical aberration of the objective part was wholly untouched, and that the eye-piece, consisting of four glasses, was achromatic."

The history of this instrument is briefly this: On a critical examination of all the four defects to which a compound microscope of the ordinary kind is liable, I judged, from theory, aided in some little degree by my own experience and that of others, that two might be wholly removed in an instrument of small volume and price, and the other two so much reduced as to produce a microscope which might prove very useful to naturalists

naturalists who could not afford to purchase very expensive instruments. As to my success I may appeal to the Doctor himself, who confesses in the next line that NOTHING CAN SURPASS THE BEAUTY OF THE FIELD OF THIS MICROSCOPE, though his firm conviction that this instrument *ought not to be good* has so far warped his judgement as to make him assert, that "nothing more can be made to grow in it [the field] than in that of any ordinary compound microscope having a well-figured object-glass of the same power, and angular aperture, used with an Huygenian eye-piece, also of equal power with that applied to the instrument in question."

Several persons have, to my knowledge, compared my instrument, which be it remembered costs but six guineas and a half, with "ordinary compound microscopes" made by the first opticians, for which they had paid twenty or thirty guineas, and have declared it decidedly superior in every respect. If the Doctor means that it is not so perfect as a compound microscope *with an achromatic object-glass*, I agree with him most cordially. Neither is a twenty-guinea telescope, though good of its kind, equal to Sir James South's twenty-foot equatorial. Let the Doctor produce me a microscope, or an *engyscope* if he likes that word better, with an achromatic object-glass, which shall magnify distinctly 360 times in linear measure, pack into a compass of about 24 cubical inches, and cost but six or seven guineas, and I shall be perfectly satisfied to yield the palm to him.

In the mean time I earnestly conjure him not to diminish the value of his own praiseworthy exertions, by incautious attacks on persons who, if they allow themselves to reply to him, may easily show that while he diverts himself with throwing stones, he forgets that he dwells in a house of glass.

I remain, Gentlemen, yours, &c.

H. CODDINGTON.

Trin. Coll. Cambridge,
July 8, 1831.

P.S. I beg leave to add that I have no pecuniary interest whatsoever in the instrument which has thus unexpectedly furnished a subject for controversy. I furnished designs to Mr. Cary, who in a very spirited manner undertook the necessary experiments at his own cost, and I certainly feel anxious that no unfounded charges should prevent him from deriving a fair remuneration from the sale of the microscope.

XIII. *On the Thermo-Magnetism of Homogeneous Bodies; with illustrative Experiments. By Mr.* Wm. Sturgeon, *Lecturer on Experimental Philosophy at the Hon. East India Company's Military Academy, Adiscombe.*

[Concluded from page 24.]

Sixth Class of Experiments.

88. *Experiments with irregular Masses of Antimony.*

THE object of these experiments was that of ascertaining if the same laws of thermo-magnetic action, as regards the crystalline arrangement of the metal and point of heat, as those which were developed in the experiments with cylinders and cones of antimony could be traced in masses of an irregular figure, by making the point of heat in various parts of those fine smooth extensive faces of crystalline laminæ which are to be met with only in fractures of large masses which have been very gradually cooled from fusion.

89. The experiments were made by heating, separately, particular points in those lamellated faces, and then tracing the direction of the electric currents by expeditiously applying the antimony to a magnetic needle, and noting minutely the character of the deflection; and it appears, from the uniformity of the results of a considerable number of experiments on various pieces, that the thermo-magnetic phænomena elicited in irregular masses have precisely the same relation to the position of the metallic films and point of heat as those displayed by cylinders and other regular forms of antimony.

90. It will not be necessary to enter into a detailed account of the several experiments which were instituted for this inquiry, as a description of those which were made on one of the irregular pieces, and of the resulting phænomena, will be sufficient to illustrate the whole. And I have every reason to believe, that the *same laws* which govern these phænomena, will be found to appertain to all similar crystalline arrangements of antimony; that they will uniformly be developed under similar circumstances, and consequently that they are intimately related to the crystallization of the metal.

91. The piece of antimony on which these experiments were made, weighed about three pounds; it was separated by the blow of a hammer from a large mass, and the fracture exposed a smooth triangular face of parallel crystalline plates, without presenting any intersecting edges of metallic laminæ whatever. This triangular face will be represented by figs. 16, 17, 18; and the arrows in those figures will show the

the directions of the electric currents as indicated by the deflections of the magnetic needle, when the point of heat was near to the angles *a*, *b*, *c*, respectively.

92. When the spirit-lamp was held for a few moments at the angle *a*, still keeping the point of the flame on the face of the fracture, the electric streams were diffused over every part of that surface *from* the point of heat towards the opposite edge; as shown by the directions of the arrows, fig. 16. Comparatively strong currents were detected in the edges *a b*, and *a c*; but in consequence of the general flow of these currents being nearly at right angles to the edge *b c*, no magnetic force could be detected when that side was held over and parallel to the needle. On leaving the face *a b c*, the electric tide swept the general surface of the metal, flowing in various directions, and returning by numerous windings to the point of heat. By this distribution, and consequent attenuated state of the electric force, the thermo-magnetic energies were comparatively very feeble on every part of the metal excepting the face *a b c*; on which alone they were displayed with promptitude and regularity.

93. When the point of heat was at *b*, the whole of the triangular face became again magnetic, displaying phænomena of precisely the same character as those which had been elicited when the point *a* was excited; and the distribution of the electric forces had again a decided reference to the point of heat; emanating therefrom, and flowing with as great an uniformity over the surface of the fracture as if it had been a conductor from the copper to the zinc of a single galvanic pair. The arrows in fig. 17. will indicate the distribution of the electric force over the surface of the lamellated fracture when the point of heat was at *b*.

94. When the fracture was heated at *c*, the thermo-magnetic phænomena were again displayed with very nice precision and uniformity on that particular face of the metal; whilst on the other parts of the surface they were confused and irregular; showing that the electric forces on those parts were dispersed in various directions, and enfeebled by their separation, or by their returning to the point of heat, through the body or general mass of the metal. Fig. 18. will show the direction of the electric tide on the face *a b c*, when the point *c* was excited by the flame of a spirit-lamp.

Remarks.

95. The uniformity displayed in the results of the preceding class of experiments confers on them a very interesting character in these investigations. In connexion with those on

regular

regular masses, these experiments establish a very important point, by exhibiting in the most striking and satisfactory manner an intimate connexion between the crystalline arrangement of the metal, and the distribution of the electric powers by heat; for, to whatever point in the flat lamellated face of this system or group of parallel scales heat was applied, the electric forces were directed over the planes of the laminæ *from* the heated point; and having traversed the general surface of the metal, returned to *that point* again, *across the edges* of the films, in precisely the same manner as in the experiments with solid cones and cylinders,—a circumstance highly demonstrative that the thermo-magnetic forces in both sets of experiments have the same specific origin, and are actuated by the same cause. The fountain of all the phænomena appears to be in the crystalline arrangement of the metal, and the direction of the electric and magnetic forces to be referable to the point of heat.

96. It very often happens that fractures such as have been described (88) (91), are bordered on some of their sides with piles or groups of laminæ, unfavourably situated for experiments of this kind; presenting their thin edges, instead of their planes, in the face of the fracture. When, however, the method of experimenting becomes known, these trifling inconveniences are not of much consequence to the uniformity of the thermo-magnetism displayed by the smooth part of the fracture under examination.

97. In the first place, the flat scaly surface on which the experiments are to be made, ought to be as extensive as possible; at least two inches across; if larger, the better. Should any side of this face present groups of the thin edges of laminæ, they may be easily removed, either by the saw or by the hammer: if those groups be not very extensive, their removal will not be necessary.

98. The principal circumstance next to be observed is, that the flame of the spirit-lamp does not touch those unfavourable crystals. The selected point of heat must always be on some part of the flat lamellated face under examination, and near to some angle. A momentary heat must suffice, and the plane immediately and dexterously applied to the magnetic needle; the deflections of which will unerringly indicate the electric current to be flowing over that surface *from* the heated point to the opposite side.

99. I have succeeded in discovering a method of forming square bars or prisms of antimony, which observe a rigid uniformity in the display of thermo-magnetic phænomena, by heating them either partially, or equably, at one end only. And

And I now find that I can predict with certainty the magnetic character of any side of the bar, by paying attention to certain circumstances connected with its casting. I have cast several sets of square bars, of an uniform size and figure, under precisely the same circumstances, and have never yet found one single bar to deviate from the general law. One of those sets consisted of fifteen bars, all of which observed the same laws of thermo-magnetic action. I have, however, in vain tried to obtain them of an uniform power, the thermo-magnetism of some of them being much more energetic than that of others. This circumstance, which I hope soon to obviate, and some others which I find associated with the display of their thermo-magnetic phænomena, but which I have not yet had time to investigate, prevents my giving a description, in this place, of the circumstances under which I have hitherto cast these prisms of antimony; the thermo-magnetic character of which can easily be predicted before the metal enters the mould*.

100. In general, these bars possess a considerable degree of power as thermo-magnets; and when four, or more of them are properly combined, their conspiring energies on the magnetic needle may be very satisfactorily exhibited to every auditor in the most spacious lecture-room.

101. I have also been enabled to cast discs of antimony, which do not vary from each other in the character of their thermo-magnetic qualities. I have not however, as yet, had time to investigate the whole of the circumstances which I suspect to be connected with the communication of that power to the metal, and therefore beg permission to reserve the detail of the experiments till another opportunity. I mention them in this place merely as facts, which I can at any time repeat. I will further observe, however, that I am of opinion that the thermo-magnetism displayed in the prisms and discs already noticed, may be traced to the same source as that displayed in other forms of antimony; that is, to the crystalline arrangement of the metal; and that electricity is intimately associated with the process of crystallization generally. This opinion is highly favoured by the well-known fact of electro-polarity being exhibited in the tourmaline and some of the crystallized gems: and as regards the metals, I imagine that the experiments and observations I have hitherto detailed, are amply

* It is next to impossible to cast bars of antimony of considerable dimensions which will not exhibit magnetic phænomena by heat; indeed, bars of almost any size, or masses of any figure whatever, whether regular or irregular, display those powers more or less. It however requires considerable attention to obtain several pieces of antimony which will observe an uniformity of thermo-magnetic action.

demonstrative

demonstrative of the connexion in that class of homogeneous bodies. And I am inclined to believe, that future labours in this curious philosophical field of research, will ultimately establish crystallography amongst those interesting sciences, which are subordinate branches, and obedient to the laws, of electricity.

103. There are, however, thermo-magnetic phænomena displayed by homogeneous metals, when experimented with in certain forms, which do not appear to be very reconcilable to the hypothesis of *electro-crystallography*. They seem to depend upon some other cause than any which that hypothesis embraces: and as they are exhibited under different circumstances to any which have yet been noticed, the experiments by which they are elicited will require to be described as a distinct class.

Seventh Class of Experiments.

104. Notwithstanding the opinions which have been set forth to show that thermo-magnetic energies are not exalted in *combinations* of metals, by employing them of large dimensions, and that a pair of particles, however small, or wires exceedingly thin, will develop the same extent of power as two bars of considerable dimensions; I was led to imagine that the same law might probably not extend to the innate magnetism displayed in homogeneous metals by heat. My inquiries were therefore directed to large masses of those metals, in which, whilst experimented on in small pieces, I was unable to discover the least trace of this extraordinary power; and the results were such as to answer my anticipations in the most ample and satisfactory manner.

105. *Experiments with large Masses of Zinc.*—The first piece of zinc in which I detected thermo-magnetic action was a rectangular cake, or flag, which had neither been rolled nor hammered. It was about 14 inches long, 8 inches broad, and ·75 inch thick, and weighed about 17 pounds. This mass of zinc, when heated at one corner only, displayed magnetic powers in a very exalted degree, and would deflect a compass needle, on which the magnetism of the earth was not neutralized, 20°, by the first impulse, when one of its edges was held in the magnetic meridian and close to the glass cover of the instrument; but in consequence of a fracture in one of its edges, the thermo-magnetic phænomena were not so nicely regulated in this piece as I have found them to be in other masses of zinc, which are uniformly sound on every side. I will therefore describe experiments which were made on a whole sound flag of zinc, weighing 42 pounds, 2 feet long, 8·5 inches broad, and about 1 inch thick.

106. The

106. The thermo-magnetic phænomena were promptly and uniformly displayed by this mass of zinc, and were precisely of the same character as those which I have observed in experiments with all similar masses that I have yet examined. They have an evident reference to the point of heat; and I believe they may be taken as a general standard for those which would be developed by all similar masses when operated on by the same process.

107. The experiments were made by heating one corner of this mass of zinc in a common fire, and keeping the other parts of the metal as cool as possible. When thus heated, the mass was held in various directions over the magnetic needle, the deflections of which were taken as an indication for the direction of the electric currents. In this manner the thermo-magnetic powers of the zinc were ascertained, whilst it was partially heated at its several angles in succession.

108. If *a*, *b*, *c*, *d*, fig. 19. be permitted to represent one of the flat faces of the zinc-plate, then the arrows in that figure will give a tolerable representation of the directions of the electric forces, as indicated by the deflections of the compass-needle when the point of heat was at the angle *d*. By contemplating this figure, it will be observed that the electric forces are projected, as it were, *from* the heated angle into the body or field of the mass, over which they become generally diffused; but separating and expanding in different directions, they sweep the surface of the metal in recurving tides towards its edges, by which routes they again return to the heated point.

109. The straight arrows in fig. 19. would seem to indicate that the electric forces in those parts of the metal were directed in right lines, which is not strictly correct. Those arrows are drawn to show the lines of *greatest energy*, or those parts of the metallic surface which, when presented to the needle, produce the greatest deflections, and are the determined resultants of the numerous curvilinear forces which are in active play during the transient disturbance of temperature in the metal.

110. It will appear evident by inspecting the figure, that on the surface of the rectangular mass, there would necessarily be two *neutral* lines, one on each side of the diagonal arrow, which would be determined at right angles to the aggregate recurving electric forces, and may be represented by the dotted lines *d n*, *d n*. These lines, are those in which a magnetic needle, unsolicited by any other force, would arrange itself, and were discovered by its uniform repose whilst situated close to those parts of the metal. The situations of the neutral lines, however, are not constantly the same; for as they are determined by

by the direction of the electric forces, and those forces again by the distribution of heat, the situations of the dotted lines *d n*, *d n*, will also vary with the circumstances of heat.

111. When one of the ends *a d*, fig. 20. of the rectangular mass is uniformly heated, the distribution of the electric forces will be indicated by the arrows in that figure. Here again, it will be observed, that the electric forces are projected *from* the heated end into the area of the plate, and by recurving sweeps, return to that end again along the parallel margins of the metal. In this case, the *neutral* lines, and lines of *greatest energy*, are parallel to each other, and also parallel to the sides *a b*, *d c* of the rectangular plane.

112. As a similar distribution of the electric forces to that represented by fig. 19. is uniformly elicited by heating any of the angles separately, the same system of arrows will serve to illustrate that distribution, to whatever angle heat may be applied. If, for instance, the angle *a* were to be heated, the points of the straight arrows *d c*, *d a*, would then be directed to *a*, or towards the point of heat; whilst the feathered end of the former would be directed towards *b*, and that of the latter towards the angle *d*. The central or diagonal arrow would be directed from the angle *a* to the angle *c*; and in the same way the system of arrows may be considered to be situated with respect to any other heated angle. The system of arrows in fig. 20. will also apply to either of the ends of the metal when uniformly heated between the angles.

113. As both faces of the zinc exhibited thermo-magnetism of the same character in all the preceding experiments, whatever has been stated concerning those experiments will equally apply to both sides, or flat faces of the metal, and I imagine to all similar masses of zinc. I must here observe, that the electric forces very seldom reach to the cold end of the mass, but approximate thereto in proportion to the advances of heat. They are the most powerful near to the heated point, and become more and more languid as they recede from it, till at length their energies are entirely lost in the more remote parts of the metal.

114. *Experiments with Masses of Copper.*—Copper is one of those metals, the thermo-magnetic energies of which are not very easily detected in separate homogeneous masses, unless they be of large dimensions. The most satisfactory results I have ever obtained from experiments on this metal were elicited by a rectangular mass, about two inches thick, and weighing about 95 pounds. This huge piece of copper, which by the interest of Mr. Marsh I was permitted to examine, belongs to the Royal Arsenal. The experiments were made

made in precisely the same manner as those described with masses of zinc; and the results, excepting in *degree*, were exactly the same in both metals. The thermo-magnetic energies were very promptly and uniformly displayed in this mass of copper, but were exceedingly feeble when compared to those developed by a mass of zinc less than half its size. With the latter metal, a needle, on which the terrestrial magnetic powers were in full play, could be made to sweep an arch of 100°; whilst with the unwieldy mass of copper, it required the soliciting terrestrial force to be entirely cut off from the needle in order to obtain a sweep of 6° or 8°.

115. There does not seem to be that uniformity in the display of thermo-magnetism by thin metallic plates as is observed to be developed by those of considerable thickness. The phænomena, when thin plates are employed, although the metal be neither rolled nor hammered, assume a very capricious character, and appear to be governed by laws which are not easily traced to any general standard.

116. I am not at present prepared to say to what cause these phænomena are attributable: they seem to be of a distinct order, and not referable to the laws of crystallization. They may possibly be traced to a difference in the progress of heat in the several parts of the metal, moving with different degrees of celerity in the margin and body or area of the mass. Should this conjecture be correct (and I have some reasons to think that it is true), I imagine that this class of experiments will exhibit a very prominent feature amongst all those, which, from time to time have been advanced for the solution of the highly important problem of terrestrial magnetism, more particularly in that branch of the inquiry which relates to the diurnal variation.

117. *Experiments with Spheres of Zinc.*—To carry the analogy of experiment still closer to terrestrial magnetic action, I have had cast, globes of zinc of different sizes, with a view of detecting some law by which their thermo-magnetic energies are exhibited when heat is partially distributed over the surface of the sphere, in imitation of the sun's action on the face of the earth. One of these globes is solid, and about 5·54 inches diameter, weighing nearly 23 pounds; another, which is hollow, and 10 inches diameter, weighs 64 pounds, the thickness of the metal being about ·75 inch.

118. With these spheres I have as yet gained but little information, owing, as I suppose, to the difficulty which I have experienced in keeping the various parts of their surfaces at temperatures sufficiently remote from each other. I have, however, succeded in deflecting a needle by applying to it the

 10-inch

10-inch globe, partially heated, to a much greater angle than any that has ever been ascribed to diurnal variation. This result, insignificant as it may appear, and far from answering my expectations as to the extent of magnetic power developed by the sphere, sufficiently warrants the prosecution of the inquiry. The experiment has demonstrated the existence of the magnetic power in a homogeneous sphere of zinc, and the development of that power by heat. The field of inquiry is thus successfully penetrated, and future investigations may possibly lead to the most interesting results.

119. A sphere of 10 inches diameter, which is the largest I can at present command, is much too small for experiments of this character. With a globe 30 or 40 inches in diameter, experiments might be made on a magnificent scale, and I apprehend with the most satisfactory results. A metallic sphere of such dimensions, with the necessary machinery for experiment, would require a sum, which perhaps but few individuals would be found willing to lay out on an inquiry which of is more of a national than of an individual interest. Researches of this nature would be the most likely to be successful were they pursued under the patronage of governments, or of wealthy scientific associations. The experiments might then be carried on under advantages the most favourable to insure regularity and uniformity in the results, provided they were conducted under the superintendance of persons who have proved themselves competent to the task. They might also be pursued to an extent which no individual could hope to arrive at, and with a success that probably might at once set this sublime philosophical problem completely at rest.

Artillery Place, Woolwich.

N.B. I have succeeded in magnetizing an iron sphere by means of a thermo-electric combination. The same sphere becomes very highly magnetic when under the influence of the electricity excited by a small galvanic pair immersed in salt water; giving *direction* and *inclination* to a magnetic needle, highly imitative of those phænomena as exhibited by the action of the earth. A description of the apparatus and mode of experimenting will be given in my next communication.

XIV. *Of the Conditions of Life. By the Rev.* Patrick Keith, *F.L.S.*

[Concluded from page 40.]

Aliment.—ALL substances capable of affording nourishment to living beings are aliments; and no living being can subsist any great length of time without the use of them.

them. If a plant is deprived of the access of the moisture of the soil, it languishes, and withers, and dies. If an animal is deprived for a length of time of all nourishment, a feeling of pain is excited in the region of the stomach, followed by faintness and loss of strength, which, without new supplies of food, would ultimately and inevitably terminate in death. As plants cannot range the fields in quest of nourishment, it was necessary that some provision should be made to furnish them with due aliment, without any effort of their own; accordingly the Creator has provided that they shall find their food in the moisture of the soil in which they grow. Their food is thus already digested, and they take it up in a fluid state by the slow and protracted process of absorption. Animals, on the contrary, having functions to perform incompatible with a stationary mode of existence, and with the assumption of food by the slow process of absorption, are furnished with the means of taking in, at certain intervals pointed out by the sensation of hunger, a competent supply of aliment in a solid state, which they have the means of digesting, and of preparing for final assimilation.—The food of plants consists chiefly in the moisture which they find in the soil, containing in solution a variety of alimentary ingredients. But we have reason to believe that they derive part of their food from the atmosphere also. The leaves attract and absorb the moisture. They inhale also its gases; and there are plants that live and thrive without any other food. The *Epidendron Flos-aëris* may be quoted as an example.—The food which animals affect is of various descriptions according to the species. Some animals are granivorous, as many birds. Some are graminivorous, as the sheep and the ox; others are carnivorous, as the lion and the tiger. Man eats almost anything, and drinks almost anything, but he likes to have his victuals cooked.

It has been thought that a line might be drawn dividing the animal from the vegetable kingdom upon the ground of the character of the food affected by each. Such, particularly, is the opinion of M. Mirbel*. Plants feed, it is said, upon unorganized substances—earths, salts, water, gases; animals upon substances already organized; that is, either upon other animals, or upon vegetables.—We do not regard it as a very good, or a very correct rule. Animals thrive well upon milk alone, which is not an organized substance. If you say that it is the product of an organized being, let it be remembered that it is also a very good food for vegetables. In short, the chief food of plants as well as of animals is either animal or vegetable substances in a state of solution; and though animals may feed

* *Traité d'Anat. et de Phys. vég.*

upon

upon substances that are still in an organized state, yet they cannot convert them into nourishment till they have destroyed their organization in the stomach. Is it certain that all animals require a food that has once been organized? What is the food of *Cancer salinus?*

A better criterion for distinguishing the animal from the plant will be found in the attribute of sensation. For though there may be some phænomena that give countenance to the idea of vegetable sensibility, yet they are not such as the physiologist can confidently rely upon; and as the attribute of sensation distinguishes the animal from the plant, so their assumption and assimilation of aliment, and their origin and mode of growth, will distinguish them both from the mineral. If this last criterion had been kept in view, the noted definition of Linnæus would have been less incorrect: "*Lapides crescunt; vegetabilia crescunt et vivunt; animalia crescunt, vivunt, et sentiunt* *,"—Stones grow; plants grow, and live; animals grow, live, and feel. But the truth is that stones do not grow in the sense in which plants and animals grow; not by the introsusception and distribution of aliment throughout their whole substance, but merely by the apposition of new particles added to the external surface. In this respect the definition is faulty. In other respects it is perhaps well enough; and in brevity and decision of tone it will not be easily surpassed.

Upon the principle which we adopted in our definition of life, namely, that of a copious enumeration of particulars, I submit the definitions that follow, with a view to mark out the boundaries of the three kingdoms of nature:—A mineral is a mass of lifeless matter, inorganic, inert, insentient; not augmentable by nutrition, but attaining its bulk merely by the external and mechanical or chemical apposition of new parts or particles.—A vegetable is a living and organized body, inert, insentient; springing from and producing a germ that is augmentable by nutrition; and fixed, by a root, to the soil, from which it absorbs its principal nourishment already in a fluid state.—An animal is a living and organized being, self-moving, sentient; springing from and producing a germ that is augmentable by nutrition, and ranging in quest of aliment, which it takes up chiefly in a solid state, and subjects to the action of digestive organs.

There are assignable limits, then, which separate the three kingdoms of nature; between the mineral and vegetable, organization; between the vegetable and animal, sensation. In an unorganized body there is no community of interests among the different parts, and no part that is necessary to the well-

* *Philosophia Botanica.* 2.

being

being of any other part. Cut or chop off any portion you please from a block of marble, and the remaining portion shall know nothing of it. In an organized body, every organ is useful to every other organ, and no organ is made for the sake of itself alone. Each sympathizes with all the rest, and each has a common interest in the welfare of the whole. The aliment which a plant or animal takes up it distributes to every member. Manure and water the root of a plant, and the leaf and flower will soon give indications that they participate in the benefit conferred; lop it severely, and the branches will suffer.—Give to an animal its due supply of food, and every organ is refreshed. Cut or chop off from it a limb, or part of a limb, and you excite a sympathy throughout the whole fabric, with a feeling of pain and of injury expressed by cries, or manifested by contortions of body.

Yet the limits separating the several kingdoms are not, in all cases, conspicuously displayed.—Look at the lower orders of vegetables—the algæ, the fungi; and in some of them it is with difficulty that you can discern even the faintest traces of organization. A mere crust adhering to the surface of a rock, as in the case of many of the lichens; or a mere mass of jelly covered with a fine epidermis, as in the case of many of the tremellæ, is all that you have for a plant. It is but little elevated above the level of the mineral.—Look at the lower orders of animals, and you find the same want of characteristic marks among them. The organization of a polype seems to be but little beyond that of a tremella: but its power of loco-motion, which is evident, and its capability of sensation, which is presumptive, are tokens that indicate its superiority to the plant.

As you advance to the higher orders of vegetables, the organization begins to be more complex, and the plant more perfect; and thus you rise through the several orders till you reach the highest and most perfect of all, producing root, stem, branch, leaf, flower, and fruit, in the perfection of their kind, and giving indications of organic and living function, and of the process of internal nutrition, in the absorption, elaboration, transmission, and distribution of alimentary fluids in peculiar and appropriate vessels. As you advance to the higher orders of animals, the organization begins to be more complicated also, and the animal more perfect, establishing the physiological maxim—that of all organized beings, whether plants or animals, the perfection of the individual is in the direct proportion of the complexity of its organization. Hence the organization of the animal is soon found to surpass in complexity that of the vegetable. Both have a variety of organs in common—tissues—the cellular, the lamellar, the vascular, the fibrous.

Both

Both have an alimentary and a sexual apparatus; for both grow and propagate their kind. Both have an apparatus for the propulsion and distribution of fluids, which they have the capacity of assimilating to their own substance.—But animals have additional faculties, and the additional faculties in question have their source in additional organs; while the organs conferred correspond to the wants of the individual. The food of the plant is already digested; but the animal has its food to digest. Hence the necessity of a stomach. The plant is stationary; but the animal moves. Hence the necessity of a muscular apparatus. The plant is insentient; but the animal is endowed with the faculty of sensation. Hence the cerebral system, the source of thought, perception, consciousness, memory, volition, loco-motion. In the lower orders of animals these additional properties are not very distinctly marked; but as you ascend the scale, they become more and more visible till at last you reach man, in whom they exist in the highest degree.

Aeration.—No living being can thrive, or even continue to exist, without the access and contact of atmospheric air. The seeds of vegetables will not germinate if placed *in vacuo.* Ray introduced some grains of lettuce-seed into the receiver of an air-pump, which he then exhausted: they did not germinate, but they germinated upon the re-admission of the air; which shows that access of air is a condition necessary to the germination of seeds*. The experiments of Homberg seem indeed to militate somewhat against this conclusion. They are recorded in the Memoirs of the French Academy for the year 1669; and the inference deduced from them is, that seeds in general do not germinate if deprived of atmospheric air; but that cress-seed, lettuce-seed and a few others will germinate even in the vacuum of an air-pump. But the same experiments when repeated afterwards by Boyle, Muschenbroek, and Boerhaave, with a much better apparatus, did not confirm the latter part of the result. On the contrary they all tended to prove that no seed germinates in the vacuum of an air-pump, and that in the cases of germination mentioned by Homberg, the vacuum must have been very imperfect. The same experiments were again repeated by Saussure the younger, who says that the seeds of peas gave indications of germination *in vacuo* in the course of four days, but never effected any development of parts beyond the mere protrusion of the radicle†.—We conclude, then, upon the whole, that in a perfect vacuum no seed will germinate; but that in the most perfect vacuum hitherto formed by human art, some seeds may germinate.

* Phil. Trans. No. xiii.

† *Saussure sur la Vég.* chap i. sect. 1.

The

The same condition is necessary to the vegetating plant. Grew having discovered, in a leaf that he was examining, a number of little bags or bladders, filled as he thought with air, drew the conclusion, and maintained the doctrine, that leaves are the lungs of plants. M. Papin, with a view to ascertain the point in question, introduced into the receiver of an air-pump an entire plant, root, stem, and leaf: the consequence was, that it very soon died. He then introduced a plant by the root and stem only, with the leaves and branches exposed to the influence of the atmosphere: still the plant died after a while; but it lived much longer than in the former case, and warranted him in concluding, as he thought, that leaves are indeed the lungs of plants*. Whether this conclusion was legitimately drawn from the premises, or not, we will not at present stop to inquire. Enough was done to show that plants cannot continue to live without the access and contact of atmospheric air. They will not even grow with vigour unless they have an abundant supply; as he who has the management of a hot-house too often discovers to his cost. The plant that grows where there is no free circulation of air springs up slender and sickly. The plant that is exposed to the action of the stormy blast springs up stout and robust.

Of the truth of the same conclusion as applicable to animals, it will scarcely be necessary to offer any formal proof. It comes so completely home to every one's own experience, that he must be a bold man who would deny it; yet if proof were wanted, it would be found in the death of many a poor mouse that has been placed in the receiver of an air-pump for the purpose of experiment.

There are, it is true, some apparent exceptions to the above rule. It has been said of the Truffle, that it vegetates without the access of air, because it vegetates wholly under ground. But it is very well known that air penetrates the soil to a depth beyond that at which the Truffle is found. It does not therefore vegetate without aëration. For the same reason it has been thought that the roots or bulbs of plants whose stem dies down to the ground in the winter must needs vegetate without air. But air is conveyed to them in the moisture of the soil; and of some of them it may be said that they hybernate rather than vegetate in the winter: at any rate they are not deprived of the access of air †. But it is in the animal kingdom that the excep-

* *Phys. des Arb.* liv. ii. chap. 3.

† [We may add, in confirmation of this reasoning, that it has been shown by Mr. Bowman, (Trans. of Linn. Soc. vol. xvi. p. 413, &c.) that the squamæ of the subterranean stem of *Lathræa squamaria* are real leaves and organs of aëration. But the same botanist remarks, that *Cuscuta*, *Listera* *Nidus*

exceptions are the most striking;—not in the department of Fishes, though their element is even the water; but in that of the Amphibia. Live toads, snakes, and lizards have been found imbedded in solid masses of stone, or of coal, at a great depth below the surface, and without any possibility of the access of air*. They are facts occurring about as often, and are about as well authenticated, as the sight of a mermaid. We cannot well refuse our assent either to the one or the other; and yet we cannot give it without a sort of sceptical reluctance. Yet if the fact in question is anything different from that of a long protracted hybernation, we are altogether without the means of accounting for it.

Temperature.—The phænomena of life have never yet been exhibited except within a certain and limited range of temperature. Too great a degree of heat, or too great a degree of cold, is equally injurious to it. At a very low temperature, as towards the poles, plants and animals are often frozen to death. At a very high temperature, as at the equator, they are apt to perish through excess of heat. But they have the capacity of preserving their due temperature under the influence of many opposing causes. In the midst of the frosts and snows of winter, plants maintain a temperature which is always above that of the surrounding atmosphere; and even under the burning heat of a vertical sun their temperature is never raised very high. But different plants affect different temperatures, and you cannot well naturalize them in climates to which they are not indigenous. You may indeed have all varieties of them in the same latitude; but it shall be in the conservatory, or in the hot-house, and if not, it shall be at different altitudes. Tournefort noticed this fact, in the case of the plants growing on Mount Ararat; and Humboldt gives us a similar account of the vegetation of the mountainous districts of South America. In ascending the Andes within the tropics, oranges, pine-apples, and all manner of delicious fruits and vegetables, are found on the lower grounds. Maize, plantains, indigo, cacao, at an altitude of from 3000 to 5000 feet. Cotton, coffee, sugar, at the same altitude, but ascending still higher. Wheat, and other European grains, together with the oak and other forest trees, at the altitude of from 6000 to 9000 feet. The pine still lingers at an altitude of 13,000 feet above the level of the sea. From 13,000 to 15,000 feet, you have grass and lichen, which last creeps up still higher,

Nidus Avis, *Monotropa*, and *Orobanche*, are destitute of true leaves, and are consequently incapable of drawing sustenance from the atmosphere. These latter plants, therefore, appear to present real exceptions to the rule here vindicated by Mr. Keith.—Edit.]

* Phil. Mag. March 1817.

higher, adhering to the surface of the porphyritic rock, and insinuating itself even under the perpetual snow*.

Similar observations are the result of our inquiries into the animal kingdom. The bear is a native of the polar regions. The elephant, lion, and tiger are indigenous only to countries near the equator. A forcible and sudden change of climate would be fatal, we may believe, to either. For although bears, lions, and elephants are found to live in countries of which they are not natives, it is only under the protection and fostering care of man. Man lives in almost all climates, but not with equal comfort. He can accommodate himself to cold climates by means of clothing; and to warm ones by going without it. The Greenlander inhabits regions bordering on 80° of north latitude, where the mercury freezes in the thermometer, that is, at 40° below zero; and yet the temperature of the blood never falls much below 96° of Fahrenheit. The negro lives under the hot and burning suns of the torrid zone, and yet the temperature of the blood never rises much above 96°. Further, plants and animals seem to be endowed with extraordinary capabilities in extraordinary circumstances. On the banks of a thermal river in the island of Luçon, the largest of the Philippines, Sonnerat found the *Aspalathus*, or African Broom, and the *Vitex Agnus-castus* growing, and, as we may suppose, thriving, though their roots were swept by the water at a temperature of 174°. In Italy Confervæ are are said to be found occasionally in the thermal springs, though heated even to the boiling point. In the above island of Luçon, Sonnerat saw fishes frolicking in a hot spring of the temperature of 158°. In the province of Quito in South America, Humboldt saw fishes thrown up from the bottom of a volcano, together with water and heated vapour, that raised the thermometer to 210°. This was quite high enough to have killed and boiled European fishes; but the fishes in question were still alive†.

Wonderful as the above relation is, it is surpassed by the following facts. In 1760 when Du Hamel and Tillet were conducting some experiments that required the heat of an oven, a girl was found who offered to go into it, to note the height of the thermometer, and who performed her task with the most perfect *nonchalance*; the mercury standing, ultimately, at 288°. The curiosity of philosophers being roused by the announcement of this fact, Sir Charles Blagden, Sir Joseph Banks, and some other men of science exposed themselves to a heat, first of 220°, and afterwards of 260°, without suffering any particular inconvenience. The pulse was quickened to 140 beats in a minute; water was boiled and beef-steaks were

* London Encyclopædia. Andes. † Library of Useful Knowledge.

dressed; and yet the temperature of the body never rose beyond 101° of Fahrenheit *. Thus there is in plants and in animals something that resists and controls the influence of chemical agents, and that something is the attribute of life. The dead animal substance, the beaf-steak, was broiled, but the living animal substance remained unaffected.

Connected with temperature we have the very singular phænomenon of the hybernation of plants and animals, that is, of some peculiar species of them; for all plants and animals do not hybernate. The state of hybernation is a state of torpidity, induced by a low temperature, and lasting till the colds of winter have gone. The living functions are suspended. In plants there is no absorption, no nutrition, no flux of juices. In animals, there is no respiration, no assumption of aliment, nor circulation of fluids; or, if this last process is at all carried on, it is in the most languid manner. Yet life is not extinguished; it is not even injured, but rather it is preserved from accidents that might be fatal to it; and when the return of spring restores again the due temperature, the individual resumes its living functions, and its hybernation ceases.

Among plants, the bulbous-rooted are said to hybernate, and the bulb is regarded as being the winter-quarters of the future plant. They do not however hybernate in the strictest sense of the term; for if you leave them in the soil for the winter, and inspect them now and then, you will find traces of the growth and development of the infant plant, even in the season of hybernation.—But the hybernation of animals is the most complete. In them the living functions are really and truly suspended, and no traces exhibited of the growth of parts. The snake, the dormouse, the swallow †, the bat, are examples of hybernating animals. They roll themselves up into the smallest compass possible, and, as it may best suit the species, take up their winter-quarters in the earth, or in clefts of rocks, or in holes of walls looking to the south. If you detect them in their hiding-place, you may handle them or pinch them or roll them about; and they shall know nothing of it till they are exposed to the influence of a gradual warmth. Their torpor is said to be the most profound at the temperature of from 5° to 7° above zero. If they are suddenly

* Phil. Trans. (abridged by Hutton), vol. xiii. p. 695.

† Much has been written about the annual disappearance of swallows; some maintaining that they hybernate; others that they merely emigrate. It is certain that such stragglers as have not joined in the general flight do actually hybernate. This I can affirm with confidence, from the fact of my having once found, in the midst of winter, a solitary swallow, in a torpid state hybernating under the thatch that covered the ridge rafter of the roof of a carpenter's shop. It revived upon being exposed to the warmth of a fire; but the weather being still too cold, it soon died.

denly exposed to a temperature that is either much lower or much higher, they are roused indeed into life, but the exposure kills them. The natural and gradual increase of returning solar heat is that which suits them the best.—Thus the attribute of life preserves them even in hybernation, and is ready to give them fresh activity when the due temperature returns.

Death.—By an irreversible decree of the original Giver of life, every living being must submit to the stroke of death, which is, as we have already observed, an extinction of all living function, and of all possibility of living function. There is no exemption, there is no escape. There is no way of eluding the grasp of this ghastly King of Terrors;—*mors nescia flecti;—mors ultima linea rerum.* It seems a hard condition, because it deprives us of all we hold dear. What a boon, what a blessing is life! And what would a man take in exchange for it?—Even vegetables seem to be conscious of its value, though we regard them as being destitute of the faculty of sensation. In the "fine frenzy" of the poet, the trees of the forest are made to rejoice, and corn-covered valleys to laugh and to sing*. Much more is the blessing of life cognoscible by animals who are endowed with organs of sense and of feeling.—See how the tender lamb frolics in the enjoyment of its newly acquired existence!—See how the birds wanton in air!—See, above all, how man appreciates the value of the gift;—and see his "longing after immortality,"—when participating, freely, in the gratifications which life presents, he reflects upon the plenitude of its delights, and mingling religion with his mirth, ascends in holy meditation to the idea of a kind Creator, and even to the glories of a future world.

However, life is liable to many accidents that tend to cut short the thread by which we hold it. Wounds, diseases, poisons, are often fatal to the life of man, as well as to other animals. A violent blow on the temples will extinguish life in an instant. Plague, pestilence, and famine, will speedily produce the like effect; and a few drops of concentrated prussic acid introduced into the animal circulation will cause almost immediate death. But if the individual should fortunately escape all fatal accidents, still a term will come beyond which life cannot be protracted; still it will be worn out at last by a natural and gradual decay.—Observe its progress in the plant, and the symptoms of approaching dissolution. The root refuses to imbibe the nourishment afforded by the soil. The juices are but feebly propelled, and their assimilation effected with difficulty. The bark becomes thick and woody, and covered with moss or lichens. The shoot becomes stunted and

* Psalm lxv.

diminu-

diminutive; and the fruit palpably degenerate, both in quantity and quality. The terminal branches fade the first, then the larger branches, and then the trunk and root. The vital energy of the fabric languishes, and is at last totally extinguished; and the plant, now exposed to the chemical action of surrounding substances which it cannot any longer resist, withers and dies away, presenting to the eye a decayed and rotten appearance, and crumbling into the dust from which it originally sprang.—Observe its progress in the animal, and you will find that the symptoms are of a similar character. It has been said indeed of man, that it is the body only, and not the mind, that suffers decay and death. But it is evident that the mind is liable to decay and to death as well as the body. If the organ perishes, its function must inevitably perish. If the brain dies, its function—mind—must cease. As well might you expect that digestion should continue when life has left the stomach, as that mind should continue when life has left the brain. If the eye becomes dim, and the ear dull of hearing, and the palate incapable of tasting, and the nostril devoid of smell; so the memory becomes weak, the judgement erroneous, the understanding embarrassed, the will slow in its decisions, and the organs that are subject to it slow in their obedience; inducing "second childishness and mere oblivion;" and exhibiting man in his state of dotage "sans mouth, sans teeth, sans eyes, sans every thing." It is but a step further to the total extinction of life, and cessation of all living function; that is, in other words, to the death of both the body and the mind.

During life, all was activity, all was vital, or spontaneous motion, all was the exercise of organic function. In plants, absorption, assimilation, and distribution of fluids, with growth and development of parts. In animals, prehension, digestion, and assimilation of food; with growth, loco-motion, intellection; and in man the faculty of speech;—all referable to the agency of that subtle, invisible, and incomprehensible something called life, which counteracts and controls mechanical and chemical agencies, and converts them to its own purposes.—If I move my arm from the pendant position, and raise it to a horizontal or upright position, the agency of gravitation is counteracted. If the materials that compose the living fabric do not tend to putrefaction, the agency of chemical affinities is counteracted. But in death there is no longer any resistance opposed to these agencies, no living action, no spontaneous motion, no exercise of organic function: in short, the fabric is no longer a living system. Chemical and mechanical agencies affect it merely, exerting themselves in their full strength, and reducing it, sooner or later, to the primordial

and

and elementary principles out of which it was originally formed.

Sleep has been said to be the image of death. But it is only a transient suspension of some of the functions of life. The exercise of function fatigues the organs, and hence they require a period of repose. Such is sleep, which lasts only till the fabric is recruited. Hybernation might be said to be the image of death also; but it depends entirely on temperature. When the temperature sinks to a given degree, hybernation begins; and when it rises again to the same degree, the exercise of function is resumed. But if death once supervenes, its dominion is perpetual; and its empire not to be escaped from. It is "the undiscovered country from whose bourn no traveller returns"—the "cheerless night that knows no morrow."

———————— Omnes una manet nox,
Et calcanda semel via lethi.— Hor. Ode 28. lib. i.

that is, as regarded in a physiological light; for the morning that is yet to "dawn on the night of the grave" we are not taught to look forward to as a consequence resulting from the established order of things, but as an event emanating from the *fiat* of the Almighty*.

Thus the phænomena of life and of death are plainly and palpably distinct. They are opposite, and irreconcilable, and cannot be mistaken. Life composes, death decomposes; life rears a fabric, death destroys it; where life extends, the integrity of the fabric is maintained; where life ends, decomposition with putrefaction begins. Such is the victory achieved by death, and the inevitable doom of every thing that lives.

Ruckinge Rectory, Kent, April 26th, 1830. P. Keith.

XV. *On the Statement in the Nautical Almanac for* 1833, *of the Time of Beginning of the Solar Eclipse of the* 16*th of July in that Year; together with the correct Times of that Eclipse, computed for Greenwich. By* George Innes, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

In the Nautical Almanac for 1833, I find the *beginning* of the solar eclipse of the 16th July, stated to be at $16^h\ 5^m$. It appears to me probable that the *units and fraction of a minute* have fallen out in printing. About four years ago, I calculated the times of the above eclipse for Edinburgh and Greenwich Observatories, using Delambre's tables of the sun,

* [John v. 28. vi. 39, 40. Rom. vi. 23. 1 Cor. xv. 36, 44, 51.—Edit.]

and

and Damoiseau's Lunar Tables of 1824. The times which I obtained for Edinburgh were inserted in Professor Jameson's Journal for October—December 1828; and the following are the results for Greenwich.

According to Burckhardt's tables the times are about 19·5 seconds *earlier*. Bessel's corrections of the Solar Tables had not reached me when I made the computations.

		Mean time.				Apparent time.		
		D.	H.	M.	S.	H.	M.	S.
The eclipse begins,	July	16	17	4	5·8	16	58	23·0
Greatest obscuration	—	—	17	55	4·2	17	49	21·2
App^{t} ecliptical conj^{n}	—	—	17	58	16·6	17	52	33·6
End of the eclipse ...	—	—	18	49	21·9	18	43	38·7

Digits eclipsed at greatest obscuration, $8^{dig.}\ 47'\ 48''\cdot 3$ on the north part of the sun's disc.

The moon will enter the sun's disc on the west limb at $37°\ 12'\ 20''$ from his vertex, in reference to the horizon.

I am, Gentlemen, &c.

Aberdeen, April 27th, 1831. GEORGE INNES.

XVI. *Errata in* Schumacher's *Ephemeris of the Distances of the four Planets Venus, Mars, Jupiter, and Saturn, from the Moon's Centre, &c. for* 1833, *published by the Admiralty. By* A CORRESPONDENT.

THE following errata have been kept back for some time, and would have been reserved for publication in the Nautical Almanac for 1834, but that the advantage resulting from the discovery of them would be diminished by the delay.

Page.									
			°	′	″		°	′	″
9.	Sept. 17. xxi^{h}.....	*for*	105	5	15	*read*	100	5	15
17.	May 2. ——......	—	38	44	29	——	88	44	29
18.	May 31. ix^{h}	—	69	3	39	——	109	3	39
46.	Jan. 22. Log. dist.	—	0·95266			——	0·05266		
51.	June 2..............	—	52	42	4	——	54	42	4
56.	November 1. November 30. Dec. S.								
63.	June 12.............	*for*	35	6	20·2	——	3	6	20·2
66.	September 20. ...	—	9	42	37·7	——	9	22	37·7
68.	November 3	—	12	33	53·9	——	12	43	53·9
75.	June 30.	Insert N.							
76.	July 31.	Insert N.							
77.	August 31.	Insert N.							
88.	July 19. Log. dist.	*for*	0·63651			——	0·69651		
98.	May 31.	—	4	35	10 N.	——	5	35	10 N.
99.	June 7. Log. dist.	—	6·97029			——	0·97029		
110.	May 11............	—	322	26	46	——	322	27	46

Chislehurst, July 1, 1831.

XVII. *On*

XVII. *On the Theory of Differences, by* Samuel Sharpe, Esq. F.G.S.*

Prop. I.—To interpolate a maximum or minimum in a series, by means of the theory of differences.

This perhaps may be useful on many occasions, in practical astronomy, when the observer has a series of altitudes at equal intervals, without all the data, such as time and latitude, necessary for the more rigid formulæ. As for instance; From a series of altitudes near the meridian, to find the meridional altitude and approximately the time of transit; which is a far more accurate method than that commonly used by sailors, of watching the sun's ascent till it becomes stationary, which gives the time of noon with extreme inaccuracy, and the meridional altitude single and uncorrected by others.

And again; From a series of declinations near the solstice to determine the solstitial declination.

To explain the signs make

Time.	Quantities observed.	First Difference.	Second Difference.	Third Difference.
m	u			
		Δu		
$m+h$	u_1		$\Delta^2 u$	
		Δu_1		$\Delta^3 u$
$m+{}^2h$	u_2		$\Delta^2 u_1$	
		Δu_2		
$m+{}^3h$	u_3			

and u' and h' the quantities sought.

By a well-known formula in the theory of differences, we have

$$u+\Delta' u = u' = u + \frac{h'}{h}\,\Delta u + \frac{h'(h'-h)}{2\,h^2}\,\Delta^2 u + \frac{h'(h'-h)(h'-2h)}{6\,h^3}\Delta^3 u \,\&c.$$

and by expanding and rearranging, [1]

$$u' = u + \frac{h'}{h}\left(\Delta u - \frac{\Delta^2 u}{2} + \frac{\Delta^3 u}{3} - \frac{\Delta^4 u}{4}\right) + \frac{h'^2}{h^2}\left(\frac{\Delta^2 u}{2} - \frac{\Delta^3 u}{2} + \frac{11\,\Delta^4 u}{24}\right) + \frac{h'^3}{h^3}\left(\frac{\Delta^3 u}{6} - \frac{\Delta^4 u}{4}\right)$$
$$\frac{h'^4}{h^4}\left(\frac{\Delta^4 u}{24}\right) \qquad [2]$$

which for convenience we will write

$$u' = u + \frac{h'}{h}\,A + \frac{h'^2}{h^2}\,B + \frac{h'^3}{h^3}\,C + \frac{h'^4}{h^4}\,D \qquad [3]$$

and if we consider this value of u as a maximum, we may make two equal values, one on each side of it; viz.

$$u_{,} = u + \left(\frac{h'+a}{h}\right)A + \left(\frac{h'+a}{h}\right)^2 B + \left(\frac{h'+a}{h}\right)^3 C + \left(\frac{h'+a}{h}\right)^4 D \qquad [4]$$

* Communicated by the Author.

 and

and

$$u_{\prime\prime} = u + \left(\frac{h'-a}{h}\right)A + \left(\frac{h'-a}{h}\right)^2 B + \left(\frac{h'-a}{h}\right)^3 C + \left(\frac{h'-a}{h}\right)^4 D \quad [5]$$

by expanding the binomials and subtracting one equation from the other, we have

$$0 = \frac{2aA}{h} + \frac{4ah'B}{h^2} + \frac{6ah'^2+2a^3}{h^3}C + \frac{8ah'^3+8a^3h'}{h^4}D$$

then dividing by $\frac{2a}{h}$, and, as a may be infinitely small, rejecting those terms which then involve a,

$$0 = A + \frac{h'}{h}2B + \frac{h'^2}{h^2}3C + \frac{h'^3}{h^3}4D$$

whence by Newton's method of reversing a series, we have

$$-\frac{h'}{h} = \frac{A}{2B} + \frac{3A^2C}{8B^3} + \frac{A^3}{32B^5}(18C^2 - 8BD)$$

when u' is a maximum; and with this value of $\frac{h'}{h}$ we shall know from our original equation the value of u' as a maximum.

As an example I shall take the following declinations of the sun from the Nautical Almanac for 1829.

	u	Δu	$\Delta^2 u$	$\Delta^3 u$	$\Delta^4 u$
June 19	23° 26′ 30″				
		+ 43″			
20	23 27 13		− 24″		
		+ 19		− 1″	
21	23 27 32		− 25		+ 1″
		− 6		0	
22	23 27 26		− 25		
		− 31			
23	23 26 55				

in which $A = +54''\cdot4$, $B = -11''$, $C = -0''\cdot38$.

D being $+0''\cdot05$ may be neglected.

$$\frac{h'}{h} = 2\cdot46 - 0\cdot31 + 0''\cdot24, \text{ and } h' = 2 \text{ days } 9^h\ 15^m,$$

being three hours wrong; and

$$u' = 130'' - 62''\cdot8 - 4''\cdot9 + u = 23°\ 27'\ 33''\cdot3,$$

being $0''\cdot5$ wrong.

These results are as accurate as could be expected, considering the original values of u do not contain tenths of a second; and if the value of $\frac{h'}{h}$ had been known from other data, the resulting value of u' would have been exact.

Prop.

Prop. II.—To determine the point of contrary flexure in a curve; or to interpolate a term in a series, such that its second difference is $= 0$ by means of the theory of differences.

Instead of $u_{\backslash} + u_{\backslash\backslash}$ as in the former Proposition, we now have $\frac{u_{\backslash} + u_{\backslash\backslash}}{2} = u'$. Consequently by expanding the three equations [3] [4] and [5] we have

$$0 = \frac{a^2}{h^2} \mathrm{B} + \frac{3\, a^2 h'}{h^3} \mathrm{C} + \frac{16\, h'^2 a^2 + a^4}{h^4} \mathrm{D}.$$

Dividing by $\frac{a^2}{h^2}$, and rejecting the terms then containing a,

we have $$0 = \mathrm{B} + \frac{h'}{h} 3\,\mathrm{C} + \frac{h'^2}{h^2} 6\,\mathrm{D}$$

$$-\frac{h'}{h} = \frac{\mathrm{B}}{3\,\mathrm{C}} + \frac{6\,\mathrm{B}^2\,\mathrm{D}}{27\,\mathrm{C}^3}$$

and hence we have in equation [1] the value of u'.

By this proposition and a series of altitudes of a star in the east or west, we may deduce the time of its passing the prime vertical, and its altitude when there.

Hence if P be the hour-angle at the pole,
L, the latitude of the place,
A, the altitude on the prime vertical,
D, the star's declination,

$\sin \mathrm{P} = \frac{\cos \mathrm{A}}{\cos \mathrm{D}}$ and $\sin \mathrm{L} = \frac{\sin \mathrm{D}}{\sin \mathrm{A}}$ (See Baily's Tables.)

Prop. III.—From a set of observations made at equal intervals to obtain one more correct, corresponding to the mean of the times. Or it may be stated, To correct the mean of a set of observations by help of the theory of differences.

Let there be seven observations; then

$$\begin{array}{lllll}
u &= u & & & \\
u_1 &= u & + \Delta u & & \\
u_2 &= u & + 2\Delta u & + \Delta^2 u & \\
u_3 &= u & + 3\Delta u & + 3\Delta^2 u & + \Delta^3 u \\
u_4 &= u & + 4\Delta u & + 6\Delta^2 u & + 4\Delta^3 u \\
u_5 &= u & + 5\Delta u & + 10\Delta^2 u & + 10\Delta^3 u \\
u_6 &= u & + 6\Delta u & + 15\Delta^2 u & + 20\Delta^3 u
\end{array}$$

neglecting the higher differences.

Now the mean of these quantities gives us

$$u_3 = u + 3\Delta u + 5\Delta^2 u + 5\Delta^3 u,$$

which is evidently too great by $2\,\Delta^2 u + 4\,\Delta^3 u$.

Hence

Hence, with seven observations,
u_3 = mean of observations -2 mean of second differences, and thus we may construct the following table:

Number of Observations.	Mean of the Times.	Quantity sought.	Value of Quantity sought.		
1	m	u ...	The observed quantity.		
2	$m+\frac{h}{2}$	$u_{\frac{1}{2}}$ ...	Mean of observation.		
3	$m+h$	u_1 ...	Do.	$-\frac{1}{3}$	mean of Δ^2
4	$m+\frac{3h}{2}$	$u_{\frac{3}{2}}$ ...	Do.	$-\frac{5}{8}$	mean of Δ^2
5	$m+2h$	u_2 ...	Do.	$-$	mean of Δ^2
6	$m+\frac{5h}{2}$	$u_{\frac{5}{2}}$ ...	Do.	$-\frac{35}{24}$	mean of Δ^2
7	$m+3h$	u_3 ...	Do.	-2	mean of Δ^2

Remark,— This proposition may be used on all occasions to which the rule is applicable, that "by a single observation is meant the mean of a series," (see Requisite Tables, Baily's Tables, &c.) and in particular in learning the time from a single altitude observed; when we consider that many persons have occasion to learn the time, who have neither instrument to observe a transit, nor leisure to take equal altitudes.

Prop. IV.—From an observed place of the moon, and four given places in the Nautical Almanac, to determine the time at Greenwich.

This is I believe not in general solved analytically, but synthetically; thus,

1st, Assuming the time at Greenwich, by help of the difference of longitude known approximately.

2ndly, By the theory of differences determining the moon's place by equation [1].

$$\Delta' u = \frac{h'}{h}\Delta u + \frac{h'(h'-h)}{2h^2}\Delta^2 u + \frac{h'(h'-h)(h'-2h)}{6h^3}\Delta^3 u.$$

3rdly, Thence correctly the assumed time.

This equation is computed more conveniently by means of tables of the coefficients of Δ, Δ^2 and Δ^3, but if such tables are not at hand would be more easily computed in the form [2].

$$\Delta' u = \frac{h'}{h}\left(\Delta u - \frac{\Delta^2 u}{2} + \frac{\Delta^3 u}{3}\right) + \frac{h'^2}{h^2}\left(\frac{\Delta^2 u}{2} - \frac{\Delta^3 u}{2}\right) + \frac{h'^3}{h^3}\left(\frac{\Delta^3 u}{6}\right).$$

But

But it would be more accurate to solve the proposition by direct analysis; and writing this last equation as before,

$$\Delta' u = \frac{h'}{h} \text{A} + \frac{h'^2}{h^2} \text{B} + \frac{h'^3}{h^3} \text{C}.$$

By reversing the series, we have

$$\frac{h'}{h} = \frac{\Delta' u}{\text{A}} - \left(\frac{\Delta' u}{\text{A}}\right)^2 \frac{\text{B}}{\text{A}} + \left(\frac{\Delta' u}{\text{A}}\right)^3 \left(\frac{2\,\text{B}^2 - \text{AC}}{\text{A}^2}\right)$$

Remark.—The rule given in the Nautical Almanac is according to equation [1], but upon the supposition that the tables are not accurate beyond $\Delta^2 u$, and consequently that $\Delta^3 u$ *ought to be* $= 0$, and therefore $\frac{\Delta^2 u + \Delta^2 u_1}{2}$ is used instead of $\Delta^2 u$: thus

$$u' = u + \frac{h'}{h} \Delta u + \frac{h'(h'-h)}{2\,h^2} \left(\frac{\Delta^2 u + \Delta^2 u_1}{2}\right);$$ but now that the tables are sufficiently accurate it would be better to use $\Delta^2 u$.

And hence it seems to me that Mr. Baily has committed an oversight; for wishing to introduce $\Delta^3 u$, he has still employed $\frac{\Delta^2 u + \Delta^2 u_1}{2}$, which was used only on the supposition that $\Delta^3 u$ *ought to be* neglected, and has written

$$u' = u + \frac{h'}{h} \Delta u + \frac{h'(h'-h)}{2\,h^2}\left(\frac{\Delta^2 u + \Delta^2 u_1}{2}\right) + \frac{h'(h'-h)(h'-2\,h)}{6\,h^3} \Delta^3 u,$$

corrected in the Errata to

$$u' = u + \frac{h'}{h} \Delta u + \frac{h'(h'-h)}{2\,h^2}\left(\frac{\Delta^2 u + \Delta^2 u_1}{2}\right) + \frac{h'(h'-h)(h'-\frac{1}{2}\,h)}{6\,h^3} \Delta^3 u$$

instead of equation [1].

XVIII. *New Optical Experiment by Professor* Airy.

[We are indebted to the kindness of a Cambridge friend for the following account of some new optical experiments made by Professor Airy: it announces some remarkable discoveries, which have an important bearing in the verification of the undulatory theory of light.]

An instructive variation of the experiment of Newton's coloured rings (suggested by the consideration of Fresnel's formulæ for the intensity of reflected vibrations) has lately been made by Professor Airy. When a lens is placed on a plane

plane glass it is well known that a set of rings is seen whose centre is remarkably black: and it is indifferent whether common light or polarized light be used, the only difference made by the latter being that when the plane of polarization is perpendicular to the plane of reflection, and the angle of incidence is the polarizing angle, the rings disappear; but on altering the angle of incidence either way, the rings still appear with the centre black. If, however, a lens is placed on a *metallic* surface, and the incident light is polarized in the plane perpendicular to the plane of incidence; while the angle of incidence is small, the centre of the rings is black; when it is equal to the polarizing angle of the glass the rings *disappear* (though there is still copious reflection from the metal): then on the smallest increase of the angle of incidence the rings are seen with their centre *white*, and they continue so till the angle of incidence = 90°. It is indifferent whether the light is polarized before or after reflection; and a remarkable effect may be thus produced: if common light is incident at an angle greater than the polarizing angle, the rings have a dark centre; but on placing a plate of tourmaline (with its axis perpendicular to the plane of reflection) between the eye and the lens, the rings are seen with a bright centre. The Professor conceives that the whole of these experiments are in the highest degree favourable to the theory of undulations with transversal vibrations as given by Fresnel, and to the idea (which is a necessary part of that theory) that polarization is not a *modification* or *physical change* in the light, but a resolution of its vibrations into two sets, one in one plane, and the other in the plane perpendicular to the former, one of which sets sometimes is suppressed and sometimes describes a different path. The last experiment (where the character of the rings is changed after they are formed) appears almost decisive of this point. From the manner in which the rings alter when the tourmaline is turned, the Professor infers that the phases of the vibrations in the plane of reflection are more accelerated by reflection at metallic surfaces than those of the vibrations perpendicular to the plane of reflection. The dark centres, it is to be observed, are never so dark as when the lens is placed on glass; and the bright ones are never very bright.

The result of the following experiment, like those of the former, was anticipated by theoretical considerations, and shows the clearness with which, by Fresnel's theory, the effects of modifications can be traced whose very nature is inexpressible on any other theory. In the common polarizing apparatus, plane-

plane-polarized light is incident, and the light emerging from the interposed crystal is resolved into two streams of plane-polarized light (by the analysing plate), of which one only is transmitted to the eye. It is known that circularly or elliptically polarized light will, if incident on the crystal, form rings; but it has not been remarked as a general theorem, that rings will be visible if the analysing plate be so constructed as to resolve the light emerging from the crystal into *any two kinds of light*, of which it suppresses one and transmits the other to the eye. Now by means of Fresnel's rhomb, or (imperfectly) by a film of mica, the analysing plate may be made to resolve the emergent light into two circularly-polarized rays, one of which it transmits to the eye, while the other is suppressed. Supposing the light to be thus analysed and supposing the incident light to be circularly polarized, theory gives this result: the tint will depend only on the gain or loss of the extraordinary on the ordinary ray: there will be no brushes: the appearances will not alter as the crystal is turned about the incident ray. These conclusions are completely supported by experiments on uniaxal and biaxal crystals and unannealed glass. Iceland spar, for instance, shows rings without brushes: nitre, &c. exhibit the lemniscates in their whole extent without any interruption.

Cambridge, July 25, 1831.

XIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

June 8th.—A LETTER was read, from Joshua Trimmer, Esq. to the Rev. Dr. Buckland, V.P. G.S. "On the diluvial deposits of Caernarvonshire, between the Snowdon chain of hills and the Menai strait, and on the discovery of marine shells in diluvial sand and gravel on the summit of Moel Tryfane, near Caernarvon, 1000ft above the level of the sea."

The object of this paper is to point out evidences of extensive diluvial action in that part of Caernarvonshire which lies at and near the N.W. base of the mountains of Snowdonia. This district is traversed in a direction from N.E. to S.W., and nearly parallel to the mountain chain, by two remarkable beds of roofing slate, well known by the name of Penrhyn Slate, dipping usually to the S.E. at a considerable angle, and extending along a series of hills of moderate elevation, between the Snowdonian chain and the Menai strait. Great part of the surface of these hills, and of the still lower ground between them and the Menai, is so covered by accumulations of drifted gravel, sand and clay, that the slate is seldom accessible, without first quarrying down

down through a thick bed of this diluvium. It occurs, not only in the valleys, but on the sloping sides and summits of hills, sometimes entirely covering the hills, at others accumulated around small projecting crags. It is spread indiscriminately, and with little reference to the rivers that now intersect the country: its greatest observed thickness is about 140ft.

A large proportion of this gravel is composed of pebbles and blocks of various sizes, derived from rocks that occur in Caernarvonshire; many of these are less rolled than pebbles of another class that are mixed with them, and which have come from a greater distance, and must have been drifted upwards by some violent inundation, in a direction contrary to that of the rivers which descend from Snowdonia into the Menai. Among these pebbles are several which can be identified with the granite, sienite, green-stone, serpentine and jasper of Anglesea: other granite pebbles agree with no rock in Anglesea or Wales, and resemble the granite rocks of Cumberland; some may have come from Ireland or the S.W. extremity of Scotland.

There are also chalk flints, which can have come from no nearer source than the chalk of the county of Antrim.

This diluvium occurs in great thickness in the lower region of the valley of the Ogwen, usually from 60 to 100ft; forming its bed, and often occupying both sides of the valley through which it flows. These sides, for a considerable distance, afford indications of having received their last form from the bursting of a lake higher up in the valley of the Ogwen.

Shells, and fragments of shells, like those on the shores of the adjacent sea, are reported by the workmen to have been found in the sand and gravel at an elevated spot near Moel Taban, on the right bank of the Ogwen, nearly opposite the quarries of Penrhyn. Mr. Trimmer did not see them here; but on the summit of Moel Tryfane, on the south of Caernarvon, towards Bethgellert, in a sinking made through sand and gravel, in search of slate, at about 20ft below the surface, he found marine shells in a bed of sand; they were for the most part broken, resembling the broken shells on the adjacent beach; when dry, they adhere to the tongue: the fragments are too indistinct to identify species; the genera Buccinum, Venus, Natica and Turbo occur among them. Mr. Trimmer found similar broken shells also in the diluvium of the low cliff near Beaumaris.

Beneath the diluvial deposits of this district, when the surface of the slate-rock is newly laid bare, it is found to be covered with scratches, furrows and dressings, like those observed by Sir James Hall on the summit of the Costorphine and other hills near Edinburgh. These furrows and dressings were noticed several years ago by Mr. Underwood: they are referred to the action of the diluvial currents which overspread the country with gravel: some of the larger blocks amid the gravel have also deep scratches upon their surface.

Where the diluvium is argillaceous, the surface of the subjacent slate has been so protected by it, as to remain sound and fit for use

as

as roofing slate up to its line of contact with the incumbent clay; but where the diluvium is of sand or gravel, admitting ready access of water through it to the subjacent slate rock, the slate is often in a shattered state, and bent and decomposed to the depth of many feet below the line of contact.

At the close of this Meeting, which terminated the Session, the Society adjourned till Wednesday the 2nd of November.

ZOOLOGICAL SOCIETY.

May 31, 1831. N. A. Vigors, Esq. in the Chair.

At the request of the Chairman, Mr. Gould exhibited a specimen of the male of the *Urogallus medius;* the *Tetrao hybridus* of Gmelin and Dr. Latham, and the *Tetrao medius* of M. Temminck.

Mr. Yarrell observed that this individual, with one other example of the same rare species, also a male, was found among a considerable number of the *Tetrao Urogallus* of both sexes, brought from Norway by a boat partly laden with lobsters for the London market. Some of the older writers considered this bird to be a hybrid produced between the *Wood Grouse* and the *Black Grouse,* and had named it accordingly: modern authors have, however, established its distinction as a species; and the female and its egg are now known. Notwithstanding the general resemblance between these two large *Wood Grouse* they are decidedly and very obviously different. In the *Tetrao medius* the beak is black; the shining feathers on the front of the neck and breast are of a rich Orleans-plum-colour; and of the 18 feathers of the tail the outer ones are the longest. In the *Cock of the Wood* the beak is white; the feathers on the front of the breast are of a dark glossy green; and the centre feathers of the tail are the longest.

The organ of voice in the *Tetr. medius* is peculiar. The *trachea* of this bird and that of the *Tetr. Urogallus* were exhibited; and Mr. Yarrell pointed out that the *trachea* of the *Tetr. medius,* eleven inches in length, has no loose fold, like that of the *Tetr. Urogallus,* but descends in a straight line to the lungs. From the thyroid cartilage two pairs of muscles follow the course of the *trachea,* one pair firmly attached to the *trachea* itself, the second pair suspended loosely in the cellular tissue. Both these pairs of muscles, after an extent of eight inches, are lost in a membranous expansion, forming a sheath, which invests the inferior fourth portion of the *trachea,* and from which sheath one muscle only on each side is sent off, immediately above the bifurcation of the *bronchiæ,* to be attached to the inner surface of the *sternum.*

The stomach is a true gizzard of great muscular power, and the intestines and *cæca,* as in all the *Grouse* tribe, are very long: the *cæca* in the present instance measured each three feet in length.

There is reason to believe that this bird inhabits the Apennines as well as the more northern localities assigned to it. Mr. Fox in his 'Synopsis of the Newcastle Museum' quotes a note of the late Mr. Tunstall which states that "he knew some old Scotch gentlemen who said they remembered, that when young, there were in Scotland both the *Cock of the Wood,* and the *Tetr. hybridus.*"

Mr. Yarrell availed himself of the opportunity to state that the hybrid *Grouse* of White's 'Natural History of Selborne' is believed to be a young *black Cock*, having nearly completed his first moult. He added that he was indebted to Mr. Sabine for the information that the *Tetr. rupestris* of Pennant's 'Arctic Zoology' has been killed in Perthshire, and that the specimen is preserved in the collection of Lord Stanley, the President of the Society.

At the request of the Chairman, Mr. Martin referred to the notes of the dissection of a specimen of *Testudo Græca* which he had laid before the Committee on the 26th of April, and stated that the correctness of these notes had been subsequently confirmed by the examination of another individual of that species, in which he had observed the same lengthened form of stomach; similar intestines; and a *cæcum* agreeing with that previously described. The urinary bladder also corresponded in form and size. The *trachea* bifurcated in the same manner; and the *bronchiæ* had the same remarkable sigmoid flexure, and were furnished with the compressing muscle which he had before noticed.

Mr. Owen remarked that he had ascertained the existence of a *cæcum* in another species of *Tortoise*, (*Emys concentrica*, Leconte,) which he had recently dissected.

The preparation of the *cæcum* of the *Testudo Græca* having been laid upon the table, it was pointed out that the part so termed in this instance consisted of a pouch formed by the oblique insertion of the small into the large intestine, the upper end of the latter being dilated as in the human subject into a *cæcum caput coli:* but that it by no means corresponded with the *cæca* of birds, and might almost be regarded as wanting when contrasted with the developement of the same part in some of the *Ophidian Reptiles*, as in the genera *Python*, *Boa*, &c.

A living individual, apparently referable to the *Gulo barbarus*, L., was exhibited. It was presented to the Society by Edmonstone Hodgkinson, Esq. of Trinidad, who describes it as being "playful and gentle, although easily excited, and very voracious. It is exceedingly strong, as is indicated by its shape; and it has the same antipathy to the water as a cat." Mr. Hodgkinson suspects that it is a native of Peru. He obtained it in Venezuela, where it was presented to him by the President, General Paez. The name he received with it was "the *Guache*;" but this appellation, it was observed by Mr. Bennett, was probably erroneously applied to the present animal, belonging rather to the *Coati*, the orthography of which is variously given as *Coati*, *Couati*, *Quasje*, *Quachi*, and *Guachi*. The latter form occurs in the 'Personal Narrative' of the Baron Von Humboldt, where it evidently refers to a nocturnal species of *Nasua*.

The form and general appearance of the animal were remarked to be altogether those of a *Mustela*, to which genus it is probable that it should be referred, together with the typical *Gulo barbarus*. A specimen of the latter was placed upon the table, from which the living animal was shown to differ by the absence of the large yellow spot beneath the neck: a remarkable distinction in this group, but on

on the occurrence of which, unless confirmed by several specimens, it was considered improper to propose regarding it as a distinct species.

A stuffed specimen and a skeleton of the *Acouchy* (*Dasyprocta Acuschy*, Illig.) having been laid on the table, the following notes on the anatomy of that animal were read by Mr. Owen.

"The subjects examined were the male and female *Acouchies* which were exhibited to the Committee on the 23d of November last by Mr. T. Bell, in whose possession they remained alive till May, when they both died in one of the remarkably cold nights of that month.

"The following circumstances were common to both animals.

"On laying open the cavity of the *abdomen* the intestines were found to be generally adherent to each other and to the *parietes* of the cavity, arising from recently effused l mph: they were also of an unusually dark colour, owing to their contents.

"The stomach consisted of a simple cavity, of a full oval shape, without any contraction between the cardiac and pyloric portions. The *œsophagus* had a course of nearly an inch within the *abdomen* before its termination. This is a circumstance worthy of notice, and which occurs in a marked degree in most of the *Rodentia*. The inner cuticular membrane of this part terminated abruptly at the *cardia*. The villous coat of the stomach was without *rugæ*, and of a gray colour, whilst that of the intestines immediately beyond the *pylorus* was stained of a very dark colour; showing that the *pylorus* had acted as a very effectual valve.

"The *cæcum* was of a capacious size, and had the same sacculated appearance as in the *Guinea-pig*; it occupied the whole of the iliac, lumbar, and part of the hypochondriac regions of the right side, and was disposed in a sigmoid form; the *colon* at its commencement followed the curvatures of the *cæcum*, and was attached to it by a continuation of the peritoneal membrane; about six inches from the *cæcum* the *fæces* became divided into pellets. The *cæcum* itself was filled by a black tough pultaceous mass, of a slightly acid odour; and the same coloured matter, but in a more fluid state, was contained in a greater or less quantity throughout the small intestines.

"The liver consisted of four principal divisions and a *lobulus Spigelii*; the gall-bladder was imbedded in a cleft in the right division, and contained a small quantity of dark-coloured watery fluid. The *pancreas* consisted of two separate lobes. The spleen was of a very dark colour, pointed at the lower extremity, and about one inch and eight lines in length.

"The kidneys were prominently situated in the hypochondriac regions, the right being nearer to the diaphragm by one half its length than the left. Each was about one inch in length and conglobate. The supra-renal glands were of an oval shape six lines by two in their dimensions, situated anterior to the upper extremities of the kidneys, but unattached to them; the right closely adhering to the *vena cava inferior*, the left to the *vena emulgens* of its own side.

"The *viscera* of the chest, like those of the *abdomen*, presented traces of general inflammatory action.

"The lungs were divided into three lobes on the left side and four on the right, the fourth being the *lobulus medius seu impar*, occupying the space between the *pericardium* and diaphragm. The heart terminated obtusely, with a slight indication of a double *apex*. The *aorta* gave off the carotids and the subclavian arteries by a common trunk.

"The rings of the *trachea* were incomplete, their extremities being separated behind by a small space.

"The cricoid and arytenoid cartilages were of large size as compared with the thyroid; the *apices* of the latter were continued into each other; the *chordæ vocales* were very short but distinctly marked, and with a small *sacculus* on each side. There were no cuneiform cartilages; the *epiglottis* was triangular with the *apex* prolonged into a small *mucro*. Viewed from above, the aperture of the *larynx* was circular, and was directed from behind forwards. The tongue was subacuminate, minutely papillate above, with a middle longitudinal line extending half an inch from the tip: it had no elevated posterior part as in the *Guinea-pig, Beaver, Hare*, &c. but at the root of the tongue there were numerous elongated cuticular processes, and on each side of the *fauces* a fold of membrane, whose action is evidently to obviate too rapid transmission of the food through the *fauces*.

"In the male the *testes* were found within the *abdomen*, with the extremity of the *epididymis* projecting through the abdominal ring; but as the whole gland could be pushed with ease through the aperture, the *Acouchy* cannot be considered one of the true *testiconda*. The *levatores penis* were very distinct, arising from the upper part of the *pubes* and terminating in tendons which ran along the convexity of the *dorsum penis* to the *glans*.

"In the female the ovaries were found of very small size and apparently in a scirrhous state.

"In both there were small clavicular bones, about the thickness of a small pin, and eight lines in length, which were connected by a ligament of the same length to the *sternum*. Their office appeared to be to afford a fixed point of attachment to a muscle arising from the transverse processes of the cervical *vertebræ* analogous to the *levator claviculæ* in *Apes*, and to give origin to part of the deltoid, by which it is better adapted to draw forwards the *humerus*."

The following notes on the anatomy of the *Thibet Bear* (*Ursus Thibetanus*, F. Cuv.) were also read by Mr. Owen. The subject examined was a young individual which had lived about two years in the Society's Garden.

"An extensive abscess was found under the *scapula*, which appeared to have communicated with the cavity of the chest; but the lungs, heart, and liver having been removed before the animal came under my hands, I had no opportunity of ascertaining the connection it had with diseases of those parts.

"The length of the animal from the nose to the root of the tail was 3 feet 4 inches: that of the intestinal canal 33 feet. Every part in the *abdomen* was loaded with fat. The stomach resembled the human in shape, and had a well marked contraction between the

the cardiac and pyloric portions; the muscular *parietes* of the latter were half an inch thick; and, as in the *Bears* generally, had a tendinous appearance externally on each side. The intestines were simply villous internally. The biliary and pancreatic secretions entered at a distance of four inches from the *pylorus.* There were four or five longitudinal *rugæ* in the terminal six feet of the intestinal canal; and the diameter was smallest at this part. There was no *cæcum*, nor any valvular apparatus in any part of the intestinal canal.

"The anal follicles were two in number, of the size of hazel-nuts. One of them was filled tensely with a yellowish-brown cheesy substance, which had a strong acetous odour; the contents of the other were of more fluid consistence, but had the same odour; the excretory orifice was just capable of admitting a common probe; the lining membrane was thin, of a white colour, but not so distinctly cuticular as is commonly found; it resembled more the lining membrane of the urinary bladder. Each follicle was surrounded by the fibres of a muscle which was inserted into the *crus penis.*

"The spleen was of a trihedral shape, 7 inches in length, $1\frac{1}{2}$ in breadth, of a light mottled pink colour and granular texture; the splenic vein contributed to form the *vena portæ* in the usual manner. The *pancreas* was of about the same size as the spleen; but the pyloric portion bent at right angles with that which passed behind the stomach.

"The kidneys consisted each of about thirty lobules. The ureters terminated separately but close together at the neck of the bladder. The urinary bladder was a narrow oblong bag, and about half an inch of the *urachus* still remained permeable from the *fundus vesicæ.*

"The tongue was long, broad, and thin at the extremity, with the edges turned down. On the upper part was a longitudinal mesial groove extending four inches from the tip. The surface was universally papillose, and with the simple *papillæ* were intermixed numerous small white petiolate *papillæ.* At a distance of five inches from the tip there were eleven large fossulate *papillæ*, forming two sides of a triangle whose *apex* is towards the *epiglottis.* Nearer to the *epiglottis* were numerous cuticular pointed processes directed backwards. The *lytta*, or worm of the tongue, was 5 inches in length, about the thickness of a crow quill, and bent upon itself near its middle part: it had fibres of the *linguales* muscles inserted into its anterior extremity, but laid loosely for the rest of its extent among the cellular texture in the interval of the *linguales* and *genio-glossi.* The *velum palati* was terminated at its lower margin by a short bifid *uvula*, the *azygos uvulæ* consisting here of two quite distinct muscles."

A pair of the middle tail-feathers of the *Phasianus Reevesii*, Hardw. and Gray, (*Phas. veneratus*, Temm.) were exhibited; for one of which the Society is indebted to the liberality of John Reeves, Esq., of Canton. These feathers measured each about five feet six inches in length. The bird from which they were obtained is the first

first individual of this rare and magnificent species ever brought alive to Europe. It was presented to the Society by Mr. Reeves, and is now living at the Garden in the Regent's Park. A second individual died on the passage to England.

XX. *Intelligence and Miscellaneous Articles.*

GENERAL SCIENTIFIC MEETING AT YORK.

THE following notice has been circulated by the Council of the Yorkshire Philosophical Society, and we are happy in complying with their request to give it publicity in our pages.

"The Council of the Yorkshire Philosophical Society having received intimation from men of scientific eminence in various parts of the kingdom, of a general wish that the friends of science should assemble at York during the ensuing autumn, we are directed to announce that the Society has offered the use of its apartments for the accommodation of the meeting, which will commence on the 26th of September, and that arrangements will be made for the personal convenience of those who may attend it. It will greatly facilitate these arrangements if all who purpose to come to the meeting would signify their intention as early as possible (by a letter, post-paid) to the Secretaries.

"WILLIAM VERNON HARCOURT, Vice-President.
"WM. GRAY, Jun.
"JOHN PHILLIPS, } Secretaries.

"Yorkshire Museum, York, July 22, 1831."

ON THE RAPID FLIGHT OF INSECTS.

In passing along the Manchester and Liverpool railway, at a speed of about twenty-four miles an hour, ascertained by a stop-watch, I observed one of the smaller humble-bees, I think the *Apis subinterrupta*, flying for a considerable distance and keeping pace with the train apparently without the slightest effort; in fact, the little traveller was going at a rate far more rapid than ours, for its accompaniment was not in a straight line but in that well-known zigzag mode of flight observable when these insects are hovering from flower to flower in search of food. Several house, blue-bottle, and horse flies were also repeated visitors: our rapid motion seemed to have no manner of effect upon them, for when it suited their purpose they darted onwards, for a few feet or yards or balanced themselves steadily over any given point; though in an instant, whenever either their efforts relaxed, or they thought it expedient to part company, they were far away in our rear. I should observe, moreover, that the wind at the time was blowing obliquely against us with a current of such strength, that I occasionally had some difficulty in keeping my hat on. Under all circumstances, therefore, of the wind's opposition and their irregular motion, I considered that the locomotive powers of these insects could not be well less

less than from thirty to forty miles an hour. Compared with the beautifully arranged muscular powers of these minute beings in the creation, how insignificant are those which science, with all its advantages, has hitherto been able to accomplish by mechanical means!

D. T.

PROCESS FOR PREPARING HYDROCYANIC ACID.

Mr. Thomas Clark of Glasgow has published an account of a method of preparing this acid, in the Glasgow Medical Journal, from which the following is extracted:

Expose some crystallized ferrocyanate of potash to a moderate heat until water ceases to be separated; put the dried powder into an iron bottle, furnished with a tube to conduct gas; the bottle is then to be put into a moderately strong fire, and to remain there as long as gas is evolved; in this operation the cyanide of iron is decomposed, but not the cyanide of potassium, this salt remaining mixed with the oxide of iron and charcoal resulting from the decomposition of the cyanide of iron; the cyanide of potassium is to be dissolved out by water, and the solution, after filtration and evaporation, is to be set aside to crystallize; the salt obtained, after drying in a gentle heat, being deliquescent, is to be kept in well-stopped bottles.

If the cyanide of iron should not have been entirely decomposed, crystals of ferrocyanate of potash will be obtained with the cyanide of potassium: they are easily distinguishable, and are to be separated.

To prepare hydrocyanic acid, of the strength proposed by Vauquelin, admitted into the last Dublin Pharmacopœia, and of about one-fourth the strength of Magendie's solution,

Take of	tartaric acid....................	72 grains.
	cyanide of potassium	32
	distilled water	1 ounce.

First dissolve the tartaric acid in [the water in a small vial; then add the cyanide of potassium and immediately cork the vial, the cork for a short time being firmly kept in by the finger; then agitate the vial in a vessel of cold water to lower the heat produced. When all action has ceased, set the vial aside in a cool and dark place for twelve hours, in order that the bitartrate of potash may form and subside; the clear solution is to be poured off and preserved for use.

VANADIUM—A NEW METAL.

M. Sefström, Director of the School of Mines at Fahlun, while examining a kind of iron remarkable for its extreme softness, discovered a substance, the properties of which differ from those of all previously known bodies, but the proportion was so small that it would have been tedious and expensive to procure enough for a detailed examination of its properties. This iron was from the ore of Taberg in Smöland, which however contained merely traces of the substance in question. M. Sefström finding that the pig-iron contained

contained a much larger quantity than the iron prepared from it, thought that the scoriæ formed during the conversion of the cast into wrought iron would be still richer: this conjecture was confirmed by experiment; and M. Sefström being enabled thus to procure a sufficient quantity of the new substance, he examined its properties, in conjunction with M. Berzelius.

The name of *Vanadium*, from *Vanadis*, a Scandinavian divinity, is provisionally given to the substance. Vanadium forms an oxide and an acid with oxygen; the acid is red, pulverulent, fusible, and on solidifying becomes crystalline; it is slightly soluble in water, reddens litmus, gives yellow neutral salts and orange bisalts. Its combinations, with acids or bases, dissolved in water, possess the singular property of suddenly losing their colour, assuming it again only at the moment of returning to the solid state; and if then redissolved they preserve their colour. This phænomenon appears to have some analogy with the two distinct states of the phosphoric acid and phosphates.

Hydrogen gas reduces vanadic acid at a white heat; a coherent mass remains, which has a weak metallic lustre, and conducts electricity perfectly; but it is not certain that the reduction is complete. Vanadium thus obtained does not combine with sulphur, even when heated to redness in an atmosphere of its vapour. Oxide of vanadium is brown, almost black; it dissolves readily in acids. The salts are of a very deep brown colour, but on the addition of a little nitric acid, effervescence occurs, and the colour becomes of a very fine blue.

Sulphuretted hydrogen, and even nitrous acid reduce vanadic acid combined with another acid, to the blue matter, which appears to be a compound of vanadic acid with the oxide of vanadium, analogous to the compounds formed by tungsten, molybdena, iridium and osmium. The oxide and acid of this metal produce other combinations, which are green, yellow and red, and all soluble in water. When the oxide of vanadium is produced in the humid way, it is soluble in water and the alkalies. The presence of a salt renders it insoluble, and upon this effect may be founded a process for precipitating it. The vanadates dissolved in water are decomposed by sulphuretted hydrogen, and converted into sulpho-salts, of a fine red colour. Chloride of vanadium is a colourless fluid, very volatile, and produces a thick red vapour in the air; the fluoride is sometimes red, at other times colourless, but always fixed. Before the blow-pipe vanadium colours fluxes of a fine green, like chrome.—*Ann. de Chim.* xlv. p. 332.

It appears that this new metal has also been discovered by M. del Rio, in a lead ore found in Mexico.—*Ibid.* xlvi. p. 205.

MAGNESIUM.

A report was made to the Academy of Sciences, on the 21st of February, on the mode adopted by M. Bussy, for obtaining magnesium in a metallic state, which is by decomposing chloride of magnesium by means of potassium. Magnesium is a brilliant metal,

of

of a silvery whiteness, perfectly ductile and malleable, fusible at a comparatively low temperature, and, like zinc, capable of sublimation at a temperature very little higher than that of its fusibility, and condensing under the form of small globules. It does not decompose water at the ordinary temperature; it oxidizes at a high temperature and is converted into magnesia, slowly when it is in rather large pieces, but when it is in fine dust it burns with great splendour, throwing out sparks like iron in oxygen gas.—*Royal Inst. Journal*, May 1831.

ON OXALIC ACID. BY M. GAY-LUSSAC.

I was aware with all chemists, that oxalic acid when heated is partly volatilized, and that the remainder is decomposed, yielding a mixture of carbonic acid and an inflammable gas*. As I was desirous of knowing more particularly the nature of the inflammable gas, I put some very pure crystals of this acid into a glass retort, which was gradually heated. At 208° Fahrenheit, the acid was completely fused; at 230° an elastic fluid was disengaged with the vapour of water; the volume of gas gradually increased as the temperature of the acid rose by the loss of the water of crystallization; from 248° to 266° the evolution of gas was extremely rapid, and it continued until the oxalic acid was completely decomposed, but with some variations of temperature, which were not precisely noted.

This ready decomposition of oxalic acid, by a very moderate heat, is the more remarkable, because unforeseen, and because the oxalic acid is considered as one of the most stable of the vegetable acids. Its decomposition by concentrated sulphuric acid, into equal volumes of carbonic acid and oxide of carbon, is not opposed to this opinion, and is readily explained by the powerful affinity of sulphuric acid for water, in consequence of which it destroys and carbonizes a great number of organic vegetable substances.

The examination of the elastic fluids, obtained by the decomposition of oxalic acid, has shown me that they are very nearly a mixture of 6 parts of carbonic acid gas, and 5 of oxide of carbon. These proportions varied but little in the course of the operation, yet towards the end the proportion of carbonic acid was rather larger.

The decomposition of the oxalic acid by a moderate heat made me suspect the intervention of sulphuric acid. I found, in fact, that in employing this acid the oxalic acid began sensibly to decompose at the same temperature as when it was alone; that is to say, at from 230° to 239° of Fahrenheit. But an essential difference is, that with sulphuric acid equal volumes of carbonic acid and oxide of carbon are obtained, as Döbereiner has observed, while oxalic acid alone gives the same gases in the proportions of 6 to 5.

This difference led me to think, that during the decomposition of oxalic acid, without the presence of sulphuric acid, another compound must be formed to explain the loss sustained of oxide of

* See Phil. Mag. and Annals, N.S. vol. ix. p. 161.—EDIT.

carbon. An experiment performed with this view showed that the water given off by the oxalic acid was acid, and that it contained formic acid. This acid appears at first in but small quantity, because it is mixed with so much water, but it distills more and more concentrated, and towards the end of the operation, when the oxalic acid is dried, it has a very penetrating smell and a sharp taste. According to the proportions found of 6 volumes of carbonic acid for 5 volumes of oxide of carbon, and supposing that it is the deficient volume of this gas, which with the assistance of water forms formic acid, it will appear that for 12 proportions of oxalic acid there is formed one volume of formic acid.

This theoretical result appeared to me to agree very well with experiment; but I did not satisfy myself of it by a direct mode. The hydrogen was unquestionably supplied to the formic acid by water, and not by the oxalic acid, for the carbonic acid and oxide of carbon ought to have been produced in equal volumes; added to which, it is a necessary consequence of the well-known nature of oxalic acid, as shown by the experiments of MM. Dulong and Döbereiner. I may remark, that if the decomposition is not urged too strongly, nearly all the oxalic acid is destroyed; no sensible quantity being volatilized.

The observations which I have now made, appear to me to render it imperiously necessary no longer to separate oxalic acid from the other combinations of oxygen and carbon,—carbonic acid and oxide of carbon; it may be arranged among the acids into which the radical enters in two proportions, and the name proper for it would be *hypocarbonic acid*, analogically with the hyposulphuric, and hyposulphurous acids, &c.; but it may perhaps be better to delay this change of nomenclature.—*Ann. de Chim.* xlvi. p. 218.

ON GALLIC AND PYROGALLIC ACID. BY M. HENRI BRACONNOT.

According to Berzelius pure gallic acid can be obtained only by sublimation; that obtained by infusion, he conceives, always contains tannin, while M. Braconnot, on the other hand, considered that procured by infusion quite pure. To determine the question Braconnot prepared some by each mode, and the results of his experiments are, that they are distinct acids; to that prepared by infusion he appropriates the name of *gallic acid*, while the sublimed acid he distinguishes by the term of *pyrogallic acid*.

Some very white gallic acid, which gave no precipitate with gelatin, was exposed to a heat insufficient to obtain any sublimate; it dissolved into a brown liquid which crystallized on cooling; it contained, in fact, much gallic acid, with an additional brown matter that gave an abundant precipitate with gelatin.

Thirty grammes of gallic acid, previously well dried, were gradually heated in a proper apparatus to obtain the sublimed acid; three grammes and a half were procured; it was very white, and yet the aqueous solution precipitated gelatin. The residue of this sublimation, redissolved in water, gave a brown liquor, which became

came much darker with persulphate of iron, and of a blueish black colour with the protosulphate of iron; these are characters which, as we shall presently see, indicate the presence of pyrogallic acid, but not that of gallic acid; the same brown liquor was abundantly precipitated into a glutinous elastic mass by gelatin. It therefore contained a different kind of tanning from that which exists in the gall-nut. It may be inferred from these results, that heat, by acting on gallic acid, occasions its elements to unite in a new order to give rise to tanning matter and pyrogallic acid. Bouillon-Lagrange has already remarked that the sublimed acid possesses characters which prevent its being confounded with common gallic acid. According to Berzelius it does not redden litmus paper; Braconnot always found it to do so. Suspecting that this effect might be derived from the tanning matter which it retains, that was separated by oxide of tin; it still however reddened litmus paper, but not so strongly as gallic acid.

The taste of pyrogallic acid is sharp and bitter; it requires $2\frac{1}{4}$ parts of water at 55° Fahrenheit, for solution, whereas gallic acid requires 100 parts at the same temperature. When sublimed a second time the greater part is decomposed, leaving a residue of tanning matter or of charcoal. Like the gallic acid it is soluble in æther; the aqueous solution is perfectly colourless; but exposed to the air it becomes gradually coloured and eventually deposits a brown matter which has the properties of ulmin, which increases more and more as the water lost by evaporation is renewed, and in some days the acid is entirely decomposed.

If a solution of persulphate of iron be poured into one of pyrogallic acid, the latter is instantly decomposed by the oxygen of the peroxide of iron, which becomes protoxide. The solution becomes of a very dark-brown colour, which by spontaneous evaporation yields a remarkable quantity of colourless transparent crystals, that may be separated from a brown matter by alcohol; these crystals are protosulphate of iron. The brown spirituous solution contains no more iron: evaporated with a gentle heat it leaves a residue, which re-dissolved in water, yields a very sour astringent solution, containing free sulphuric acid and a tanning matter which precipitates gelatin abundantly.

The protosulphate of iron gives a blackish blue colour to a solution of pyrogallic acid. If into an aqueous solution of the same acid only a very little of the persulphate is put, so as to decompose only a part of the acid, the protosulphate of iron produced gives eventually a blue colour to the solution. These reagents act in a very different manner with gallic acid; for the persalts of iron always give a fine blue colour with it, while the protosalts occasion no change.

When nitrate of silver or protonitrate of mercury is added to an aqueous solution of pyrogallic acid, the whole of the metal is instantly precipitated in the metallic state.

A saturated solution of pure gallic acid in water is not rendered turbid by nitrate of silver; after some time the solution becomes

 brown,

brown, and metallic silver is precipitated; with protonitrate of mercury it gives an orange yellow precipitate, which becomes gradually of a dirty green colour.

When pyrogallic acid is slightly heated with concentrated sulphuric acid, it does not impart any particular colour, and is not sensibly decomposed, which is rather a remarkable circumstance. Purified gallic acid was treated in the same way, with the intention of discovering a tanning matter: the acid assumed a fine purple colour, which disappeared on the addition of water, and crystallized gallic acid was precipitated. If the solution of gallic acid in sulphuric acid be exposed to a greater heat, part of the purple colour yet remains; but almost the whole of the gallic acid is converted into a powder of a fine brown colour, which has the characters of ulmin, and without the development of tanning matter.

The compounds of pyrogallic acid with bases were not examined, except the pyrogallate of alumina, which is easily obtained by dissolving freshly precipitated gelatinous alumina in pyrogallic acid. A very acerb liquor is formed, which is rendered turbid by heat, and becomes transparent again on cooling, precisely like acetate of alumina. It produces an exceedingly abundant white coagulum with gelatin. The pyrogallate of alumina is capable of being crystallized. It appears to redden litmus paper even more strongly than the acid itself, as if the alumina in this case also acted like an acid. The gallate of alumina also possesses properties similar to those just described.

According to the opinion expressed by Berzelius, that which chemists have imagined to be pure gallic acid contains much tannin; endeavours were made to combine the latter with the sublimed acid, with the intention of reproducing a substance similar to gallic acid; but every effort was unsuccessful.

From the observations which have been detailed, M. Braconnot concludes:

1st, That gallic acid obtained in the humid way, and properly purified by animal charcoal, may be considered as pure and unmixed;

2ndly, That when it is exposed to heat it is converted into a tanning matter and pyrogallic acid;

3rdly, That on combining the latter with tannin, gallic acid cannot be reproduced.—*Ann. de Chim.* xlvi. p. 206.

ANALYSIS OF TENNANTITE.

To the Editors of the Philosophical Magazine and Annals.

If the subjoined analysis of an ore of *Tennantite* copper from a recently opened mine is worthy of insertion in the Philosophical Magazine, it is at your service. I confess, I regret deeply that the mineralogical knowledge which was formerly attainable from your magazine has recently fallen off. Why do not Dr. Turner, Mr. Heuland, Mr. Brooke, and others, step forward to communicate the knowledge they possess and keep up the zest for so interesting a science?

science? Well may collectors complain that the world is dead to the science—well may dealers complain that they have no inducement to collect—when the science is suffered to die away for want of encouragement from those who ought to promote it by the dissemination of that information which they possess.

I remain, yours, &c.

AN OLD CORRESPONDENT.

Analysis of a specimen of *Tennantite* from Trevisane Mine in the parish of Gwennap, in Cornwall. By J. Hemming, Esq. Vice-President of the London Mechanics' Institution, and Lecturer at that Institution, and also at the Russell and London Institutions.

Silica	5
Copper	48·4
Arsenic	11·5
Iron	14·2
Sulphur	21·8
	99·19

Can any of your Correspondents give any account of the chemical tests for the new metal 'Vanadium'? or the process by which Mr. Johnstone discovered what he calls the Vanadiate of lead, from the Lead Hills, and, as he says, Alston Moor,—but more probably from the neighbourhood of Keswick?

[Mr. Brooke seems to have anticipated our correspondent's complaint, as will appear from the present Number. A notice of Vanadium will be found in p. 151.—EDIT.]

LIST OF NEW PATENTS.

To C. M. Hannington, Nelson Square, Surrey, gentleman, for an improved apparatus for impressing, stamping, or printing, for certain purposes.—Dated the 22nd of January 1831.—6 months allowed to enrol specification.

To L. Schwabe, Manchester, manufacturer, for certain processes and apparatus for preparing, beaming, printing, and weaving yarns of cotton, linen, silk, woollen, and other fibrous substances, so that any design, device, or figure, printed on such yarn, may be preserved when such yarn is woven into cloth or other fabric.—22nd of January.—6 months.

To R. Winch, Gunpowder Alley, Shoe Lane, London, printer's joiner, for certain improvements in printing machines.—29th of January.—6 months.

To J. Bates, Bishopsgate-Street-Within, London, esq., for certain improvements in refining and clarifying sugar. Communicated by a foreigner.—31st of January.—6 months.

To J. C. Schwieso, Regent-street, London, musical-instrument maker, for certain improvements on piano-fortes and other stringed instruments.—2nd of February.—6 months.

LUNAR

LUNAR OCCULTATIONS FOR AUGUST.

Occultations of Planets and fixed Stars by the Moon, in August 1831. *Computed for Greenwich, by* Thomas Henderson, *Esq.; and circulated by the Astronomical Society.*

1831.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean solar time.	Immersions. Angle from North Pole.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean solar time.	Emersions. Angle from North Pole.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Aug. 1	*f* Tauri ...	5·6	379	23 37	14 57	133	96	0 39	15 59	269	238
2	71 Tauri ...	5·6	503	22 24	13 40	94	54	23 18	14 34	298	258
	θ^1 Tauri ...	5	510	23 29	14 45	136	96	0 21	15 37	256	218
	θ^2 Tauri ...	5·6	511	23 25	14 41	115	75	0 25	15 41	277	239
	80 Tauri ...	6	515	0 26	15 42	17	339	☽ almost touching Star.—Occulted to places further North.			
	81 Tauri ...	5·6	517	0 31	15 48	35	357	0 50	16 6	359	322
	(99) Tauri	5·6	516	0 37	15 53	164	126	1 11	16 27	230	192
	Aldebaran	1	528	3 15	18 30	151	133	4 6	19 22	241	236
3	111 Tauri	6	640	22 0	13 12	25	349	22 12	13 24	357	320
	115 Tauri	5·6	651	22 59	14 11	147	108	23 37	14 49	237	197
8	Mercury ...	...	...	4 38	19 30	121	82	5 26	20 17	215	175
11	γ Virginis*	4	{1465 / 1466}	18 3	8 45	42	81	Under	horizon.	...	...
29	48 Tauri ...	6	468	2 5	15 34	107	78	3 17	16 47	289	277

* DoubleStar.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1831.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·241. June 20. Wind S.W.—Min. 29·681. June 11. Wind W.
Range of the mercury 0·560.
Mean barometrical pressure for the month .... 30·001
Spaces described by the rising and falling of the mercury............ 3·640
Greatest variation in 24 hours 0·315.—Number of changes 24.
Therm. Max. 74°. June 22. Wind W.—Min. 48°. June 6. Wind N.
Range 26°.—Mean temp. of exter. air 61°·37. For 31 days with ☉ in ♊ 60·68
Max. var. in 24 hours 21°·00.—Mean temp. of spring-water at 8 A.M. 50·48

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 11th.... 90°
Greatest dryness of the atmosphere, in the afternoon of the 3rd ... 40·
Range of the index .... 50·
Mean at 2 P.M. 54°·5.—Mean at 8 A.M. 60°2.— Mean at 8 P.M. 68·
—— of three observations each day at 8, 2, and 8 o'clock .... 60·9
Evaporation for the month 5·30 inches.
Rain in the pluviameter near the ground 1·555 inch.
Prevailing winds, S.W. and W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 16; an overcast sky without rain, 6½; rain, 4½.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
22	14	28	0	27	17	17

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
4	1	0	2	½	9	8½	5	30

General

General Observations.—This month has been alternately dry and showery; but from the powerful influence of the solar rays in June, the showers of rain were seasonable, and aided the growth of the crops.

In the morning of the 9th at nine o'clock, a large double halo appeared round the sun, with two parhelia outside of the exterior halo; the inner edge of the interior halo was twenty degrees distant from the sun's centre, and the inner edge of the exterior halo was 22° 35′, while each of the parhelia was twenty-six degrees from his centre, and they had white trains seven or eight degrees long.

In the night of the 12th there was sheet-lightning from ten P.M. till three A.M., when it was accompanied with thunder and heavy rain; the lightning, which was vivid and of a blueish colour, had nearly ceased to discharge itself from the clouds when the rain came on.

In the afternoon of the 14th two parhelia appeared at the exterior edge of a large solar halo, from half-past four P.M. till sunset.

Hay-making began here about the middle of the month: the hay was ricked in dry order, and was found to be average crops, and more in some places. The wind having prevailed nearly the whole period from the western side of the meridian, there has been no arid, but rather a moist air, which, with much sunshine, has had a beneficial effect on the growth of the wheat, so much so, that with a fortnight's fine, dry weather it would be fit for the sickle. Comparatively speaking the barley and oats are rather backward in growth, yet, like the wheat, they have a promising and plentiful appearance.

The mean temperature of the external air this month, is equal to the mean of June for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are, four parhelia; one lunar and six solar halos; four meteors; one rainbow; thunder on one day and lightning on two days; and five gales of wind, namely, four from the South-west, and one from the West.

REMARKS.

London.—June 1. Fine. 2. Fine, with a very dry atmosphere. 3, 4. Fine. 5. Fine: rain at night. 6—8. Fine. 9. Fine: slight rain at night. 10. Showers. 11. Fine: rain at night. 12. Cloudy. 13. Showers. 14. Very fine. 15. Fine, with showers. 16. Fine. 17. Showers: fine. 18. Overcast. 19. Rain: fine. 20—23. Very fine. 24. Cloudy: fine. 25. Cloudy: rain at night. 26. Rain: clear. 27. Fine: heavy showers in the afternoon. 28. Fine. 29. Wet. 30. Cloudy.

P.S. The barometrical observations could not be taken on the 22nd, the day of the Fete. The columns are consequently left blank. The thermometers, being in a different place, were registered.—R. T.

Penzance.—June 1. Fair. 2—4. Clear. 5. Fair. 6—8. Clear. 9. Fair: rain. 10. Fair: showers. 11. Fair: rain. 12. Fair. 13. Fair: rain at night. 14, 15. Fair. 16. Clear: showers. 17. Fair: showers. 18. Fair: rain. 19—21. Fair. 22, 23. Clear. 24. Fair. 25. Fair: rain. 26. Fair. 27. Fair: showers. 28—30. Fair.

Boston.—June 1, 2. Fine. 3. Fine: very heavy dew early A.M. 4. Cloudy. 5. Cloudy: rain P.M. with thunder. 6—8. Cloudy. 9, 10. Cloudy: rain P.M. 11. Stormy: rain early A.M. 12. Cloudy: rain early A.M. 13. Cloudy: showers A.M. and P.M. 14. Cloudy. 15. Fine: rain with thunder P.M. 16, 17. Fine. 18. Cloudy: brisk wind. 19. Cloudy: heavy rain early A.M. 20, 21. Fine. 22. Cloudy. 23. Fine. 24, 25. Cloudy: rain early A.M. 26. Rain. 27. Fine. 28. Rain. 29. Cloudy: rain A.M. and P.M. 30. Cloudy.

Meteoro-

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* Giddy *at Penzance, Dr.* Burney *at Gosport, and Mr.* Veall *at Boston.*

Days of Month, 1831.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	$8\frac{1}{2}$ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	$8\frac{1}{2}$ A.M.									
June 1	30·063	29·925	29·90	29·84	30·048	29·909	29·50	74	41	66	50	66	51	60	NE.	NE.	NE.	E.	0·30	...	...	...	...
2	30·132	30·111	30·08	30·00	30·134	30·117	29·62	75	43	66	52	70	52	61·5	W.	SE.	N.	W.	·35	...	...	...	...
☾ 3	30·163	30·124	30·10	30·08	30·196	30·159	29·65	77	48	68	51	65	56	59	NW.	NE.	SE.	W.	·25	...	...	...	...
4	30·182	30·161	30·12	30·10	30·212	30·179	29·67	76	48	68	52	72	52	58	N.	NE.	W.	N.	·25	...	...	...	...
5	30·117	30·037	30·15	30·10	30·181	30·100	29·52	75	50	69	51	73	52	64·5	W.	NW.	W.	NW.	·20	0·10	...	...	...
6	30·087	30·076	30·05	30·02	30·105	30·065	29·57	62	49	68	55	63	48	53	N.	NE.	N.	N.	...	...	...	...	0·10
7	30·074	29·902	30·00	30·00	30·116	29·965	29·58	63	46	66	56	65	49	52	N.	SE.	N.	N.	...	...	...	...	...
8	29·834	29·811	29·95	29·90	29·899	29·839	29·27	71	46	65	48	68	53	55	NW.	NW.	W.	calm	·50	...	...	...	...
9	29·861	29·834	29·88	29·74	29·922	29·856	29·24	76	54	62	54	69	56	61·5	SW.	S.	SW.	NW.	...	·05	0·310	0·090	...
● 10	29·748	29·722	29·70	29·70	29·800	29·792	29·06	72	55	66	54	67	58	61	SW.	SW.	SW.	N.	...	·07	·105	·100	·06
11	29·758	29·715	29·72	29·70	29·807	29·681	28·93	74	55	64	55	67	58	64	W.	SW.	SW.	W.	·50	·13	·450	·145	·30
12	29·897	29·772	30·00	29·80	29·957	29·832	29·20	74	55	65	54	70	55	63	W.	W.	W.	calm	...	...	...	·400	·10
13	30·084	29·886	30·10	30·00	30·082	29·922	29·31	76	51	67	53	68	56	64·5	SW.	SW.	W.	W.	...	·06	...	·030	·07
14	30·148	30·056	29·80	29·80	30·159	30·060	29·52	78	52	66	56	67	57	65	SW.	SW.	SE.	NW.	·40	...	·100	...	...
15	29·873	29·822	29·79	29·78	29·904	29·844	29·27	76	52	68	55	70	55	64·5	SW.	SW.	SW.	NE.	...	·05	...	·075	...
16	29·836	29·815	29·78	29·78	29·872	29·832	29·23	74	52	65	53	66	56	62	SW.	W.	SW.	W.	...	...	...	·005	·12
☽ 17	29·985	29·923	29·84	29·80	30·012	29·956	29·43	73	54	65	53	70	57	63·5	SW.	SW.	SW.	W.	·40	·02	·220	·045	...
18	30·007	29·974	29·84	29·80	30·002	29·909	29·33	69	58	65	55	73	59	65	SW.	SW.	SW.	W.	...	...	·580	·310	...
19	30·052	29·948	30·00	29·80	30·099	29·962	29·30	70	58	65	53	70	53	64	SW.	W.	SW.	SW.	...	·13	...	...	·27
20	30·191	30·175	30·08	30·02	30·241	30·221	29·55	68	45	67	53	69	53	64·5	SW.	SW.	SW.	W.	·60	...	...	...	...
21	30·200	30·162	30·08	30·08	30·219	30·184	29·55	76	50	68	55	67	53	66	SW.	SW.	SE.	W.	...	...	...	...	...
22	......*	......*	30·15	30·10	30·227	30·205	29·54	78	56	64	54	74	55	67	SW.	N.	W.	calm	...	...	...	...	...
23	30·216	30·114	30·18	30·15	30·224	30·181	29·60	83	49	66	52	73	57	64·5	SW.	N.	N.	calm	·60	·05	...	·020	...
24	29·930	29·859	30·10	30·00	30·015	29·917	29·25	72	52	64	51	68	57	59	W.	NW.	W.	W.	...	...	...	...	·04
○ 25	29·789	29·672	29·98	29·85	29·855	29·732	29·16	64	51	60	54	64	54	61	W.	W.	NW.	NW.	...	·22	·090	·160	·15
26	29·828	29·646	29·88	29·82	29·885	29·699	29·06	68	46	64	56	66	51	56·5	W.	NW.	NW.	NW.	·50	·07	...	·080	·05
27	29·900	29·696	29·90	29·90	29·932	29·831	29·33	71	53	63	54	70	55	62	SW.	NW.	W.	W.	...	·30	·110	·060	...
28	30·000	29·920	30·02	30·00	30·024	29·920	29·40	63	53	62	53	65	53	57	N.	N.	N.	NW.	...	...	...	·010	...
29	30·015	29·996	30·08	30·05	30·037	30·007	29·48	64	52	64	54	67	53	54·5	W.	N.	NW.	N.	...	·12	...	·025	·17
30	30·073	30·011	30·10	30·10	30·058	30·005	29·43	70	54	63	53	70	56	55	W.	NW.	NW.	N.	·45	...	...	...	·37
	30·216	29·646	30·18	29·70	30·241	29·681	29·38	83	41	69	48	74	48	60·9					5·30	1·37	1·965	1·555	1·80

* See preceding page.

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

SEPTEMBER 1831.

XXI. *On Isomorphism.* *By* H. J. Brooke, *Esq. F.R.S. L.S. & G.S.**

SOME new and important theoretical doctrines relative to the chemical composition of minerals have, within a very few years, been advanced, and very generally adopted, on the continent. They have also been favourably received by some persons in this country, apparently on the credit of the proposers, and without much, if any, inquiry into the merits of the doctrines themselves.

The theory of Isomorphism was first introduced by Mr. Mitscherlich, and was founded upon the observation of a very near agreement in the forms of crystals of certain chemical compounds. Thus sulphate of iron and sulphate of cobalt crystallize in oblique rhombic prisms, and apparently of the same measurements; and hence the atoms or molecules of the oxides of iron and of cobalt are assumed to be *isomorphous*, or to possess the same *identical* forms; and the same apparent agreement having been found to subsist among the arseniates and phosphates of lead, the molecules of the arsenic and phosphoric acids are also assumed to be isomorphous.

From other observations it appeared, that barytes, strontian, and oxide of lead ought to be isomorphous; and hence that the salts of those substances, when produced by the same acid, ought also to be isomorphous.

But on examining the sulphates and acetates it was discovered that their respective angular measurements were not alike,

* Communicated by the Author.

alike, and they were ascertained therefore not to be strictly isomorphous. The sulphates are right rhombic prisms, and a corresponding dihedral angle of each afforded the following measurements:

Sulphate of barytes	....	101° 42′
—— strontian	....	104
—— lead	....	103 42

It became necessary therefore that the doctrine of isomorphism, in the strict sense of the term, should as a *general* principle be abandoned; and it is not unreasonable to conclude that the crystals which suggested the theory, and which appear to measure alike, may really differ in some small quantity which the goniometer does not detect.

But although the doctrine of isomorphism, or absolute *identity* of form, cannot be supported, it has been said that the forms in each respective case belong to the same *system of crystallization*, and they have therefore been termed *plesiomorphous* by Mr. Miller, of Cambridge, in a paper on some artificial crystals, read to the Cambridge Philosophical Society, in March 1830; and if even the *class* of primary form can be indicated with certainty by the chemical composition of a crystallized body, a benefit will so far have been conferred on science by the theory of Mr. Mitscherlich.

But the doctrine of isomorphism has been carried much further by Mr. Rose, and other eminent chemists on the continent, and has given rise to a theory, from which some check to the progress of mineralogical chemistry is greatly to be apprehended.

It is well known to mineralogists, that there is a considerable diversity in the chemical composition of some minerals which have been regarded as belonging to the same species. Among the most remarkable examples are amphibole, pyroxene and garnet.

Before the theory of isomorphism was proposed, it was conceived that amphibole was composed of some invariable elements in definite proportions, although, from the disagreement in the analysis of different specimens, neither the nature nor the proportions of those elements had been accurately determined.

It was also supposed that the differences in the actual composition of different specimens were occasioned by the accidental mixture of extraneous matter with the real constituent elements of the mineral. This view is taken by Haüy, and is illustrated in reference to amphibole as follows:

Analysis

Analysis of a *black* variety from Cabo de Gato, a *green* from Zillerthal, and a *white* from St. Gothard.

	Black.	Green.	White.
Silex	42	50	28·4
Lime	9·8	9·75	30·6
Magnesia	10·9	19·25	18
Alumina	7·69	0·75	
Oxide of iron	22·69	11	

But the matrix of the *black* contains a considerable portion of alumina, that of the *green* much magnesia, and the *white* is found in magnesian carbonate of lime; and hence the disagreeing composition of the three varieties may, he says, so far be accounted for.

That well-known mineral the Fontainbleau sandstone afforded an example of a mixture obvious both to the sight and the touch. But the form being that of carbonate of lime, it has been deemed a crystal of that substance mixed with a very large proportion of siliceous matter; and if the quartz had been present in very much smaller grains, or even in elementary particles, there is no reason to conclude that the mixture might not equally have taken place; for small quantities of silex frequently occur as foreign matter in minerals, and are so regarded even by the advocates of isomorphism, and there does not appear any absolute limit to the proportion that may be so involved.

Haüy appears, therefore, to have been warranted in ascribing the differences of composition of amphibole, from different localities, to mixtures of foreign matter derived more or less from the bed or matrix in which the crystals were produced.

The theory, however, which Rose and others have engrafted on the doctrine of isomorphism in its *strict* sense, is, *that the supposed isomorphous elements may become substitutes for, and replace each other, in any indefinite proportions, without varying the form of the mineral in which the substitution has taken place.*

But the truth of isomorphism in simple binary compounds having been disproved, the very foundation of the theory of isomorphous substitution would appear to be destroyed; and it will be afterwards seen, that this theory both wants the direct support of facts, and leads to contradictory results.

In the Annals of Philosophy for June 1826, is a classification of minerals by Berzelius, founded, as he says, upon the *property of isomorphous bodies to replace one another in indefinite proportions.*

 According

According to this classification 1 *atom of silica* may combine in *eudyalite* with 1 *atom of soda* or *lime* or *zirconia* or *protoxide of iron* or *protoxide of manganese*, without any change of crystalline form; or it may combine with either of these alone, or, according to the law of replacement or substitution, with several of them, or even all together, in any indefinite proportions. In *fetstein* it may combine with 1 *atom of soda* or *potash.* In a particular kind of *garnet*, with 1 *atom of lime* or *magnesia* or *protoxide of iron* or *protoxide of manganese.* In *pyrope*, with 1 *atom of lime* or *magnesia* or *protoxide of iron* or *oxide of chrome.* Hence *soda*, *potash*, *lime*, *magnesia*, *protoxide of iron*, *protoxide of manganese*, *zirconia* and *oxide of chrome*, may mutually replace each other in their combinations with 1 *atom of silex*, without affecting the form of the crystallized compound; and hence according to the theory they are strictly *isomorphous* atoms.

Paranthine is said to consist in part of 2 *atoms of silex* combined with 1 *of soda* or *lime;* and *stilbite* appears composed in part of 3 *atoms of silex* combined with 1 *of soda* or *lime.* But if *soda* and *lime* are isomorphous in relation to 1 or 2 or 3 atoms of silex, there is not any obvious reason why all the other elements that are deemed isomorphous in relation to 1 *atom* should not be equally so in relation to 2 and 3 *atoms;* and as it appears that some of these are isomorphous in respect to *alumina* also, when that substance acts the part of an acid and replaces the *silex*, as it may do according to this theory, it follows that the others must also be so; and hence there is introduced a *play* of composition into the chemical constitution of minerals, which in the happiest manner puts an end to the chemical distinction of species.

Berzelius has given the chemical constitution of some varieties of *amphibole*, as follows:

Grammatite......	1 atom of	*tri-silicate of lime,*
	and 1 ——	*bi-silicate of magnesia.*
Actinolite........	1 atom of	*tri-silicate of lime,*
	and 1 ——	*bi-silicate of magnesia,*
	or of	*protoxide of iron.*
Hornblende	1 atom of	*tri-silicate of lime,*
	and 1 ——	*bi-aluminate of magnesia,*
	or of	*protoxide of iron.*

But if the doctrine of isomorphous substitution is to be regarded as a *general* principle, these can be only particular cases of a more general formula; for it is not, I suppose, pretended by the theorists, that atoms which are isomorphous in respect

respect of *stilbite* and *paranthine* are not so in relation to *amphibole;* and if they are so, then *amphibole* should consist of .... 1 atom of *tri-silicate of X,*
or *tri-aluminate of X,*

and 1 atom of *bi-silicate of X,*
or *bi-aluminate of X.*

X being any or all the eight elements given above, and to which others might be added, in any imaginable proportions.

M. Beudant, who admits the doctrine of isomorphous substitution, has in his Mineralogy limited the general formula expressing the theoretical composition of *amphibole* within still narrower limits than those of Berzelius; for he restrains the composition of *actinolite* to single atoms of *tri-silicate of lime* and *bi-silicate of iron*, and that of *hornblende* to *tri-aluminate of lime* and *bi-aluminate of iron.*

There is not, however, a single analysis of any variety of amphibole that I have yet seen, and there are nearly twenty published by Leonhard in his *Handbuch*, and by Bondsdorf, as given in the Annals of Philosophy for October 1822, which affords the slightest ground for M. Beudant's imaginary composition of hornblende. Nor is there a single analysis that corresponds with exactness to any of the other formulæ of Berzelius or Beudant. The reader may therefore satisfy himself, by comparing the theoretic formulæ with the published analyses, that the theory of isomorphous substitution is not in respect of amphibole supported by the observed facts; the evidence of which ought, on the contrary, in reference to so fundamental and important a change in the doctrines of chemistry, to have been both distinct and conclusive.

It must also be recollected that the chemical formulæ which the doctrine of isomorphism has been brought forward to reconcile and support, are in very many instances, as their author Berzelius has candidly acknowledged, little more than theoretical assumptions founded upon the actual results of analysis; the essential together with the accidental or foreign matter, being all taken, and parceled out in such proportions as are required by the atomic theory for producing definite compounds. But in doing this there is generally some intractable surplus of one or more of the elements, which is left out and is therefore regarded even by the new theory as intrusive matter. The results of analysis may also occasionally, as Berzelius has remarked, be equally well represented by two or more different formulæ, according to the proportions which may be taken of the actual ingredients, and the manner in

in which these may be distributed in forming binary compounds; and hence what might appear as isomorphous substitution under one formula, might become foreign matter under another, which equally well represents the original analysis.

But even if the varying results of analysis had supported the new theory, it might fairly have been inquired, whether the known disagreements in chemical composition could not be accounted for upon any other probable supposition.

Now the obvious one of accidental mixture, which is so apparent in the Fontainbleau sandstone, might probably have rendered any new theory unnecessary,—for the observation and the statements of Haüy already given might generally have furnished an explanation of the disagreements in question; yet even if those disagreements could not in all cases have been explained by reference to the matrix, but that the mineral should be found to contain matter which is also foreign to that matrix, such matter might still have been present at the time the mineral was formed, and hence might have become enveloped in it.

But the supporters of isomorphous substitution will probably say, that all which might be considered foreign matter, ought to appear in the mineral in indefinite quantities, and not in those near approaches to definite proportions which have given rise to the new theory. This argument is not however really opposed to the supposition of the disagreements being occasioned by mixture of foreign matter.

I shall suppose, for the purpose of illustration only, that amphibole consists essentially of a single atom of tri-silicate of lime, and that all else which might be discovered by analysis is accidental mixture. But it may be said, that the magnesia and iron and manganese which are present in amphibole are frequently combined nearly in definite proportions with the silica, and perhaps as tri-silicates, affording a presumptive agreement in favour of *substitution.*

But if we suppose the proportion of silica present when the mineral was formed to have exceeded that which was necessary to combine with the lime in the production of our supposed amphibole, there does not appear any ground for concluding that the remainder of the silex might not combine in definite proportions with the other matters that were present, although foreign to the constitution of amphibole; that the same circumstances which determined the composition of a *tri*-silicate in the amphibole, might not occasion the production of *tri*-silicates of the foreign matter; and that the new and

and extraneous silicates thus produced might not be cemented together and cased up, as it were, by the true amphibole, in the same manner as the sand in the Fontainbleau mineral: and if this were the case, the result of analysis would be such as it now appears, approaching more or less nearly to definite composition, as the different foreign ingredients happened to be present in quantities more or less nearly corresponding to definite proportions.

But the theory of isomorphous substitution appears to involve the much more general chemical law, *that any indefinite number of binary compounds may combine together in all possible proportions;* that 1 atom of silicate of lime may, *not merely mix*, but *combine* with 100 or 1000 atoms of silicate of soda, together with as many of silicate of potash, and a dozen other substances. For the doctrine of substitution implies, that if 1000 atoms of silex should exist in circumstances favourable to the production of a compound mineral, and that in the formation of such mineral there should be required 1000 atoms of lime to be combined with the silex, and only 999 atoms should be present, these would, according to the existing doctrines of chemistry, combine with 999 parts of the silex, and the remaining part would combine with a particle of magnesia or iron, or some other matter supplied from the other elements surrounding it. But this separate binary atom must, according to the new theory, become at the same time *chemically* associated with the 999 parts of silicate of lime; for it would otherwise be simply *mixed*, as any other foreign matter accidentally present might be, and would not then be a constituent part of the mineral, as the theory of substitution requires.

But an inquiry here presents itself, whether, if the differing atom be only *plesiomorphous*, it may still act as the substitute for the atom of lime; and if it may, would the crystal so formed be *isomorphous*, or only *plesiomorphous.*

M. Beudant appears to adopt the theory of *isomorphism*, in its *strict* sense, in those compounds in which silica and alumina act the part of acids; but in relation to certain carbonates, he assumes the theory of plesiomorphism, and ingrafts upon it a new theory of his own, relative to the dependence of the angle of the plesiomorphous crystal upon the number of its substituted or replaced atoms. He says, in page 60 of his treatise on Mineralogy, that where the carbonates of lime, iron, or magnesia exist together in a mineral in any proportions, the angle of its primary form is an *arithmetical mean* of the angles of the primary forms of its component parts.

Thus

Thus if the primary rhomboid of carbonate of lime and magnesia, consisting of 1 atom of each, measure 106° 15′

And if carbonate of lime measure...................	105°	5′
Carbonate of magnesia must measure..............	107	25
together......	212	30

the half, or 106° 15′, being the angle of the compound.

Again, if a mineral contain 10 atoms of carbonate of lime, and 1 of carbonate of magnesia, its angle may, according to this theory, be found as follows:

10 times 105° 5′, the angle of carb. of lime, is	1050°	50′
once ... 107 25, the assumed angle of carbonate of magnesia	107	25
together......	1158	15

of which 1-11th, or 105° 17′ 43″, should be the angle of the compound mineral.

But this theory is at variance with accurately measured crystals, and has doubtless arisen from some error in M. Beudant's experiments which has escaped his notice.

A mineral from Zillerthal named *Breünnerite* may be adduced as an instance of disagreement with M. Beudant's theory. The mineral was analysed by myself, and afterwards by Stromeyer, and found to consist of

Carbonate of magnesia	86	or 9 atoms	nearly.
Carbonate of iron......	14	or 1 atom	

If the angle of carbonate of magnesia be, as before given, 107° 25′, that of carbonate of iron being known to be 107°, the angle of the compound must obviously, according to M. Beudant's theory, be *less* than 107° 25′, whereas it has been found by repeated measurements to be 107° 30′.*

But the theory of isomorphous substitution appears also to be at variance with itself by affording contradictory results.

Thus it is said by Berzelius that *paranthine* may be composed of *bi-silicate of lime* and *silicate of alumina*, or *bi-silicate of soda* and *silicate of alumina*. Hence these two compounds are regarded as isomorphous, and may produce *square prisms of paranthine*. But *sodalite*, whose crystals are regular dodecahedrons, are composed as the *soda paranthine* it is said may be; and hence the *two paranthine* compounds may give dissimilar forms, and the elements lime and soda *are* and *are not* isomorphous.

Again, according to the formula given by Berzelius to express the composition of *eudyalite*, *zirconia* being isomorphous

* See Phil. Mag. and Annals, N.S. vol. i. p. 397.—Edit.

with

with the substances already enumerated, ought therefore to replace or be replaced by any of them without occasioning any change in the form of the compound. Therefore *eudyalite*, whose form is *a rhomboid*, might be composed wholly of *silicate* of *zirconia*, a compound which occurs as *a square prism* in *zircon*; or it might be composed wholly of *silicate of iron* or *magnesia*, which is the composition of *olivine*, whose crystalline form is a *right rhombic prism*. Hence the same chemical formula, denoting a *silicate* of any or all of nine elements which are assumed to be *isomorphous*, represents the composition of at least three different minerals, whose respective forms are a *square prism*, a *right rhombic prism*, and a *rhomboid*.

But there are other difficulties in the way of isomorphism, which do not appear to have been considered, and which arise out of results that are at variance with the assumption of any fixed relation between the crystalline form of a mineral and the forms of its constituent molecules.

The crystalline form of sulphur is either a right or an oblique rhombic prism, according to the circumstances under which it crystallizes. Hence, unless some new element not hitherto discovered enters into the composition of one of these forms, the crystalline form of even a simple substance depends upon the mode of arrangement of the molecules in the crystal as well as upon their figure.

The crystalline form of silver, of copper, and of bismuth, is a cube, and the sulphurets of these metals, (as sulphur must be regarded as isomorphous in respect of each,) might be expected to present similar forms. But the sulphuret of silver is a cube, that of copper a right rhombic prism, and that of bismuth also a right rhombic prism, but differing in its angle from the sulphuret of copper.—Arsenic combines with sulphur in two different proportions, and producing different primary forms. Hence the *proportion* of a *common element*, and therefore an isomorphous one, occasions a change, even in the *system of crystallization*.

Sulphuret of silver and sulphuret of lead are both cubes, and hence silver and lead should be isomorphous. But chloride of silver is a cube, and chloride of lead a rhombic prism.

These remarks are thrown together for the purpose of calling the attention of those by whom the theory has been perhaps over-hastily received, to a closer investigation of its merits; that if it be really founded on sound principles, its apparent inconsistencies may be explained, and if otherwise, that it may not remain an impediment to the progress of analytical research into the true chemical composition of minerals.

 XXII. *Ex-*

XXII. *Exposition of a New Dynamico-Chemical Principle. By Mr.* John James Waterstone*.

"The new discoveries, in short, reveal to us the world of secret motions, whose laws are probably analogous to those of the universe, and which deserve to be the subject of our most earnest meditations."

Œrsted on Thermo-Electricity, Edin. Encyc.

WHEN we reflect on the progress which has been made, and is still making, in the physical sciences, and more especially in those which investigate the active properties of matter; when we behold that insatiable thirst after discovery, that enlightened spirit of inquiry, which so universally pervades the philosophic world, it becomes a source of exalted gratification to trace the steps which have led to so many brilliant results, and in contemplation of the future to look forward to that period when all that is now concealed under the veil of mystery shall finally be exposed in the sublime grandeur and simplicity which so eminently characterizes the works of nature. The illustrious example which Newton held forth to posterity, of a philosopher who applied mathematical reasoning with so much success in explaining the grander phænomena of the universe, introduced the same system amongst those who succeeded him; which, joined to experimental analysis, have unfolded a series of the most splendid discoveries in every department of natural philosophy. Heat, electricity, magnetism, and light, are the principal fields in which the powers of induction have been most conspicuously displayed. The late discoveries and researches of Young, Œrsted, Seebeck, &c. have shown that those sciences are intimately connected, and that the actual principles of nature, if we except perhaps gravitation, interfere with each other in such a manner as to lead us to conjecture they may all be particular modifications of one agent. If we, however, consider the numerous insulated facts which experimental investigation is so fertile in producing, that cannot even be generalized under any special laws, or included under any common analogy, we must be sensible that a vast distance yet separates us from the primary causes of all those phænomena.

Experiment, however ably conducted, has as yet shown nothing in heat, electricity and magnetism, but simply and exclusively the existence of force, and it seems doubtful if it will ever lead us directly to the knowledge of the essential nature of those powers. Heat is an example of a repulsive energy existing between the constituent atoms of bodies, and all the

* Communicated by the Author.

the different situations in which it and various substances are placed in relation to each other, serve only to exhibit instances in which the quantity and intensity of the repulsive power varies, thereby enabling experimentalists to deduce general laws which govern a diversified series of phænomena.—Electricity and magnetism, on the other hand, present still more curious instances of invisible forces exercising functions which rival gravitation in the important parts they sustain in the œconomy of nature. Modern discoveries have developed the intimate nature of their connections; and the display of polar forces being their most remarkable feature, is so peculiar to them alone, that we are induced to look upon them as modifications of the same elementary principle. These powers are likewise connected with heat and light, which latter are in most cases coexistent.—Electricity is the basis of chemical affinity, and heat is either absorbed or evolved in chemical action; the former is in most cases accompanied with a simultaneous change in the latter, whilst the latter in peculiar circumstances induces electrical phænomena.— Magnetism is weakened or destroyed by an excess of heat; whilst the more refrangible rays of light possess evident magnetic properties.

Thus are all those subtle agents connected together in the different effects which they communicate to matter, and in the variety of forces which harmonize the routine of natural phænomena. Gravitation alone appears to be excluded from this system of interference; no difference in its intensity, as far as experiment has yet shown, being consequent upon any change which may take place either in temperature or in electric state. Are we then to conclude that heat and electricity are removed from the sphere of its action? that they act independently of its general influence? If such is their relation to each other, our conceptions of relation and quantity are violated, and the nature of their existence must not only be different but contradictory to every thing of which we can form the remotest conception. Experiment, however, warrants no such conclusion; the process appears too delicate, and the magnitude of the results may, like the parallax of the fixed stars, be placed far beyond the range of our means of observation, without at the same time justifying doubts which may be entertained either of the intimate connection of gravitation and caloric, or of the real magnitude of these heavenly bodies.—Is it not more consonant to reason to consider them as inseparably combined in their operations? We know that both powers surround every particle of matter, exist in the same space, and generate motion, at the same time and in the same place: may we not therefore with justice conclude, that so far from acting independently, gravitation may communicate to heat all the pro-

 perties

perties by which it is distinguished, and that they may both be examples of the same elementary force exhibited through different media?

Such conjectures, although derived from an extensive and minute survey of the facts elicited by experiment, are yet prevented from ever becoming of practical utility, by our ignorance of motion. How does it originate? In what manner are its effects so complex, and its development so varied? These are questions which first naturally arise, and indeed comprehend all that can form the object of philosophic inquiry. The first, relating to its origin, has often been investigated both by physical and metaphysical authors: many abstract speculations have engaged the attention of the latter, but they universally partake of the usual defects of that science, referring every effect to a primary cause, which in whatever way it may be defined in words, fails to convey any definite conception or satisfactory meaning to the mind. Disgusted with such idle and inconclusive reasoning, and despairing of being practically useful to science by prosecuting such inquiries, it is not surprising that philosophers have never devoted themselves to investigate this subject on simple physical principles, by the application of which alone we may expect to further the progress of natural philosophy.

Two opinions are entertained by philosophers relating to this subject: the first, that the absolute quantity of motion in the universe is always the same, suffering neither the smallest increase or diminution; the second, that "motion is much more apt to be lost than got*," and that therefore "some other principle is necessary for conserving it," to supply the continued loss incurred "by reason of the tenacity of fluids and attrition of their parts, and the weakness of elasticity in solids." The former doctrine was maintained by the Cartesians, who defended their opinions by the aid of such extravagant hypotheses, that Newton and his followers, by showing the obvious absurdity of their demonstration, adopted the contrary belief, not so much from the satisfactory proofs brought forward in support of it, as because it was contradictory to the great principle of Des Cartes, which was naturally supposed to have partaken of the general fallacy of his vortical system. Later philosophers, although ardent admirers of Newton and his philosophy, have yet rejected his doctrine, and in refutation of it have brought forward mathematical proofs of its fallacy, which if not conclusive, are at least plausible and ingenious. To prove either hypothesis however, involves reasoning distinct from the abstract comparison of quantities. An intimate ac-

* 30th Query, Newton's Optics.

quaintance with nature is requisite. The constituent principle of the attractive and repulsive powers, and their mode of operation, may differ from all we can deduce in comparison. When we therefore perceive matter, once in a state of motion, gradually arrive at a state of rest, without any visible transference of its power, having no direct proof to the contrary, we are induced to consider it absolutely lost. Still the following simple analogy appears to afford evidence of the contrary, and authorizes the conclusion, that momentum like matter cannot by natural means be annihilated, the existence of both being of equal importance in the œconomy of nature. A body when falling towards the earth, gradually accumulates a quantity of momentum, which is visibly lost when it arrives at the ground. In this instance the momentum, before it is transferred to the falling body, is invisible; why may not, therefore, the same momentum after collision, be again reduced to the same invisible state without being actually destroyed? The manner in which it appears and disappears is certainly different, but the latter may be governed by laws as unalterably fixed as those of the former, although from the complexity of the attending circumstances their influence cannot so readily be appreciated. Thus after collision, in the above example, undulations or vibratory motions are always observed to take place. These changes are influenced by the nature of the composing substance, which again is an immediate consequence of the peculiar molecular forces of the ultimate constituent particles. Since we have this reason to suppose that their molecular forces are, like gravitation, subject to fixed laws, and are every way of like importance and universality, it becomes highly probable that they are alone the invisible agents which abstract this momentum of collision, without any evidence of its existence being afterwards perceived.

These views of the transference of motion are further deserving of attention by their accordance with the simplicity of nature, and by tending to clear science of all those auxiliary causes, the introduction of which, though necessary to explain the contrary hypothesis, has yet proved a serious obstacle to the progress of true philosophy. If founded on truth, they induce a lively hope that matter and motion alone will be found sufficient to explain all the phænomena attending the grand cycle of nature's operations, and that that system of unity and simplicity which the advancement of discovery is always bringing further into view, will at length be completely unfolded, and all the physical sciences eventually traced to the varied development of these two principles.

Two

Two opinions are at present entertained of the origin and nature of gravitation. In the first, no intermedia are deemed necessary to convey its influence; whilst in the second, direct impulsion is considered essential, and a subtle fluid or æther is supposed to transmit the power from one region of space to another. This last doctrine has been reckoned by some unphilosophical, by introducing a clumsy mode of explaining that, which certain refined metaphysical speculations on causality do not require to be explained. But although the cause which is sought may not on these metaphysical principles be necessary, yet it will always remain inconceivable how two bodies in an absolute vacuum will move towards each other in accordance with the laws of gravitation; and it is certainly preferable to adopt the contrary opinion, more especially if we discover a certain arrangement of the fluid which will explain the development of an attractive and repulsive energy on the most simple and evident mechanical principles.

In the following three articles it has been attempted to show, how an attractive force may exist between two particles proportional to the quantity of matter in each, and which is in every other respect subject to laws similar to those of gravitation. As it is intended at present to introduce and explain the general principle alone, without entering into mathematical details, the systematic arrangement which would otherwise become necessary, will not be so particularly attended to as briefness in demonstrating what will be sufficient to convey a distinct notion of the system; reserving for a future opportunity its mathematical elucidation and further extension in explaining a diversified series of chemical and electrical phænomena. The following particulars define what is intended to be understood as properties of matter coexistent with perfect solidity, and are the foundation of all the reasoning afterwards made use of.

Postulates.—Let it be granted that,

1st, Perfect solidity is accompanied with an inseparable union of parts.

Many may deny this as an unwarrantable assumption; but although hypothetical, it is but a corollary to the doctrine which is at present supported by the most enlightened and distinguished philosophers, who have inferred from the combining ratios of the simple chemical elements, that matter is divisible to a certain extent only, after which no force is capable of effecting any change in the relative situation of its parts; and that when a plenum exists within the surface of the ultimate particle, no disunion of parts can be effected*.

* See Phil. Mag. vol. lxii. p. 360; lxiii. p. 372.—Edit.

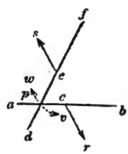
2nd, Per-

2nd, Perfect solidity is accompanied with perfect elasticity. This proposition, although inserted in the form of a postulate from being dependent on the foregoing, is yet capable of being demonstrated. If after collision two elastic bodies recover their shape with a force equal to that by which they have been compressed, they will recoil from each other without any alteration being effected in the sum of their motions. This will likewise happen, however much we consider the extent of compression to be enlarged or diminished; perfect elasticity being always consequent to an equality existing betwixt the force of the contracting and that of the dilating vibration. This vibration may be therefore conceived to be infinitely reduced; the body will then be *perfectly* solid, and this finally becomes the limit of perfect resiliency.

Exposition of a principle by which it is proposed to explain the manner in which a mutual attraction may exist between two particles of matter by the direct impulse of an intermedium.

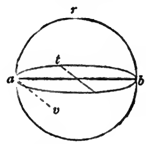
I. Let there be an infinite number of particles of *cylindric form*, the length of each being indefinitely greater than its breadth; and let them be extended through space at finite equal distances from each other; and let an indefinite velocity be then communicated to each, which may cause them to assume a rectilineal motion in different directions. What will be the after state of the medium so constituted?

Each rigid line will pursue an undeviating course, until it meets with another moving in a contrary direction, when a collision will take place, and by 2nd postulate a perfect reflexion, without the sum of their motions being diminished, although the whole momentum will then be stored up in the particles in a different manner, *a considerable portion being gradually abstracted to effect a rotatory movement*, whilst the rectilineal velocity becomes greatly diminished. Thus let the line ab, having at first a motion in the direction cr, impinge against another df, having likewise a rectilineal motion in a contrary direction es. Let p be the point of concourse: then by dynamical formula $\frac{fd^2\ 6\cdot2832}{12\ ep}$ and $\frac{ab^2 \times 6\cdot2832}{12\ cp}$ are the spaces which would be afterwards described by e and c respectively during one revolution of the line fd, ab; and the ratio of the rotatory to the rectilineal momentum, will therefore be $\frac{1\cdot5708\ ep}{\cdot5236\ df}$ and $\frac{1\cdot5708\ cp}{\cdot5236\ df}$ if no elastic force is supposed



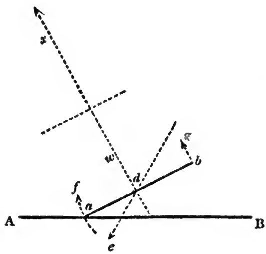
to

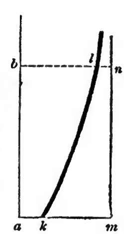
to be exerted. Since perfect resiliency however occurs, the force of impact will be reflected in contrary directions, and the supplementary momentum will be exerted at p on $f d$ in the direction $p v$, and on $f d$ in the direction $p w$ in both lines; it will therefore tend to augment the rotatory, and simultaneously lessen the rectilineal motion. The actual ratio which these quantities will have to each other, after the condition of the medium is established, will be influenced by the following circumstances; 1st, It is equally probable that the point of concourse p may be anywhere situated in the lines $a b$, $d f$; the extreme cases are, when it coincides with their extremities or centres of gravity: in the former by the above formula the rotatory motion is $\frac{3}{5}$ths of the whole; in the latter it is 0; the mean quantity or $\frac{3}{10}$ths will, therefore, show the ratio of the whole quantity of rotatory momentum generated in the medium by this cause singly, and $\frac{7}{10}$ths that of the remainder, by which the lines continue their rectilineal motions. 2ndly, The rotatory motion by the diversified concourse of the particles will be performed simultaneously in planes perpendicular to each other. For while $a b$ revolves in the plane $a r b$; another particle may communicate a rotatory impulse in any other direction $v a$, which by a well-known principle in dynamics will cause the line $a b$ to revolve at the same time in the planes $a t b$, $a r b$, perpendicular to each other. In this manner the whole quantity of rotatory momentum effected by the first cause will be nearly doubled, whilst the elastic recoil will tend further to diminish the rectilineal motion of the particles. Before ascertaining exactly this ratio, the principles of chance require to be employed in estimating the frequency of peculiar modes of concurrence, and thus discovering the mean results of the combined action of the whole medium. It is not intended, however, to enter upon this investigation at present, as it is unnecessary to prove the truth of what has been advanced, that the *primitive momentum is separated into two parts, one of which is employed in sustaining a rectilineal and the other a rotatory motion.*



II. Let a rigid plane be introduced into this medium: What will be the corresponding change in its relative density? Let the line $a b$, having a rectilineal motion in the direction $d e$, and a rotatory motion $h g$, impinge against the plane of which A B is a section; the whole momentum in the line will immediately after the first collision be separated into 1st, a reflecting impulse in the direction $a f$; and 2ndly, the influence of the

the remaining force in continuing the motion towards A B, and bringing the other extremity b likewise in contact with it, thus inducing a contrary rotatory motion, which by encountering and balancing the former will concentrate the whole force in the centre of gravity, and thus cause the line to shoot forwards from A B in radiating lines $w\,x$; nearly the whole momentum being now exerted in a rectilineal direction. Thus the lines after collision with the plane A B will convey a newly acquired impulse radiating from every point of its surface, and the rectilineal force thus generated will be communicated from one part of the medium to another. For we may suppose the fluid surrounding the plane to be divided into concentric films, and the primary rectilineal impulse occasioned by the introduction of the plane to be communicated from the first to the second, from the second to the third, and outwards successively. After the mutual reflection which takes place between the first and second, the former will have given away a portion of its rectilineal force, whilst the supplementary portion is converted into a rotatory motion. The same interchange will take place between the second and third films, whilst at every collision an absolute loss of rectilineal momentum will be incurred, which will continually replace the vibratory reflections of the first film on the rigid plane. The radiating influence will be thus conveyed through boundless space, its intensity diminishing in a ratio of the distance. The balance of forces which before the introduction of the plane A B had preserved the homogeneity of the medium will be now destroyed, and the density of its relative parts will be proportional to the intensity of the rectilineal velocity of the component particles. Thus the difference between the density of the fluid at any point surrounding the plane, and of the same before the introduction of the plane, will decrease in a ratio expressed by some function of the distance of that point from the centre of impulsion. This ratio if *linearly* developed will be represented by a curve $k\,l$ originating at a finite distance $a\,k$ from the axis $a\,b$ expressing the density of the medium at the surface of the plane at a, and gradually

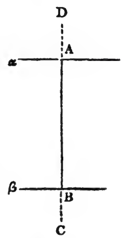




approaching the asymptote $m\,n$, which is parallel to $a\,b$; their mutual distance $a\,m$ being proportional to the density of the fluid at an infinite distance from a, or to every part of it before the introduction of the rigid plane.

III. Let a second rigid plane be introduced and placed at a distance from the first, indefinitely greater than the extent of its longitudinal dimensions. What are the effects consequent on this arrangement?

Let A B be the relative position of the two planes, which let first be considered parallel, and intersected by a common perpendicular A B. Let α A, β B, represent their indefinite extension. The rareness of the medium occasioned by the impulsive force radiating from the plane being proportional to the quantity of rectilineal force exerted amongst the particles in the same space, and the sums of the intensities of the rectilineal motion proceeding from both planes being greater in the interior space A B than in B C A D, on the exterior sides, it follows that the medium will be denser in the latter than in the former, *and more particles will thus impinge in the same time on the exterior than on the interior face of each.* The equilibrium which kept the first plane at rest before the introduction of the second will be therefore destroyed, and a motion communicated to each, which will cause them mutually to approach with an accelerating velocity; and this by the decomposition of forces will likewise take place at whatever angle the particles are inclined to each other. *Thus the effect consequent on this new arrangement will be the development of a mutual attractive force.*



Observations. — The foregoing principle is founded on a simple mechanical effect, which may be made the subject of experiment. Take a small glass cylinder, suspend it by a fine thread, and communicate to it a spinning motion round that centre. Bring it now gently in contact with a glass plate; the instant that collision takes place the cylinder will be thrown from the plate with considerable violence, whilst its revolving motion will have almost totally disappeared. This experiment, which is easily performed, corroborates what is mentioned in the first part of Article II. and affords a satisfactory proof of the efficacy of the general principle. As a corollary to Articles II. and III. we have to observe, that if the finite particles were rigid planes of uniform thickness, the attractive power at the same distance would be proportional to the extent of surface; in other words, to the quantity of matter contained in the

the particle. Again, since the intensity of all radiating influences are inversely as the squares of the distance, it is extremely probable, although not yet demonstrated, that the attractive power generated by the above principle will likewise follow the same law. When the distance, however, is so small that the peculiar shape of the particles will modify the effect, polar forces will be developed, and the planes will gradually arrange themselves in a parallel direction, while other peculiar changes relating to chemical phænomena will simultaneously take place. These modifications of the influence of the medium when the mutual distance of the rigid planes is small, will be more complex and interesting according to their number and mode of clustering together. Thus if we were to suppose every particle to be composed of an indefinite number of rigid planes arranged in a fixed order, and kept in their relative places by the balancing influence of an attractive and repulsive force, an accordance may be discovered with the theory now generally adopted, that the atomic weights of all the chemical elements are simple multiples of that of hydrogen; and that the particular properties of the particles of every different substance result from their individual organization.

The introduction of another medium, the particles of which may be considered as indefinitely larger than those of the first, and yet indefinitely smaller than the elementary plane, opens a new and wide field for the display of an unlimited number of curious phænomena. These, as far as the subject has as yet been explored, appear to coincide remarkably with the known properties of heat, whilst other simple combinations are likewise successful in explaining many other interesting facts in chemistry. Before entering, however, into this boundless region of inquiry, the circumstances which influence the variable ratio of the attractive force, and other preliminary theories, must be subjected to a course of mathematical examinations;—this will perhaps form the subject of a future communication.

In conclusion it may be observed, that were the size of the particles indefinitely reduced, whilst their velocity was indefinitely augmented, the density of the medium would be simultaneously diminished, whilst the quantity of force existing in a finite portion by remaining constant may be conceived sufficient to impel bodies with powers of equal intensity to those which are exhibited in nature; and this more especially since we may likewise suppose the rigid planes which constitute these bodies to be indefinitely reduced in thickness, whilst they still present the same superficies: thus by lessening the quantity of matter acted upon, the intensity of the action will be proportionably more vivid and efficacious. Thus were

the velocity of the particles such as would carry them in void space from one extremity of the planetary regions to another in a second of time, the ratio of their magnitude and mean distance will approach that of the stars which form a nebula; whilst the mean space described in the medium by a particle before the direction of its motion is altered by impingeing against others, may be indefinitely smaller than any conceivable magnitude. However extreme the rapidity of this action may be considered, it is yet finite, and cannot therefore be reckoned irrational or contrary to the institutions of nature. It is the true primitive standard to which all velocities [upon the hypothesis] may be compared, as from it every other motion in the universe is derived.

Professor Œrsted, as far back as 1813, in his work on the Identity of Electricity and Chemical Affinity, deduced, from an extended series of experiments and general view of science, "that all effects are produced by a fundamental power operating in different forms of action." It is remarkable that the principle we have been explaining is in perfect unison with the sentiments of this eminent philosopher. A medium is supposed to envelope the universe, every particle of which is a minute reservoir of power, which is conserved in the rectilineal and rotatory motion with which it is endowed. These motions, by the introduction of the gross particles of different substances, are alternately transformed into each other; and thus the primordial power of Œersted's doctrine by the varied structure of the particles of matter "operates in different forms of action," and is everywhere developed in the diversified series of natural changes.

St. John's Hill, Edinburgh, April 14, 1831.

XXIII. *An Examination of* M. Virey's *Observations on Aëronautic Spiders, published in the* Bulletin des Sciences Naturelles. *By* John Blackwall, *Esq. F.L.S.**

The *Bulletin des Sciences Naturelles* for July 1829, p. 131—134, contains a notice, from the pen of M. Virey, of my memoir on Aëronautic Spiders, printed in the Transactions of the Linnæan Society, vol. xv. part ii.; and it is particularly deserving of attention, that the author, by an extraordinary misapprehension, originating apparently in an imperfect acquaintance with the English language, not only distorts the facts I have promulgated and perverts the arguments founded upon them, but even attributes to me opinions the

* Read before the Linnæan Society, May 4, 1830; and communicated by the Author.

very

very reverse of those advanced in my paper. Of the great injustice done to me in that article I have every reason to complain, were I disposed to give way to personal feelings; my sole object, however, in animadverting upon it is the promotion of science, being impressed with the belief that the principal errors into which M. Virey has fallen, if suffered to remain unnoticed, must tend to retard its progress. That I should be thus forced into collision with a distinguished fellow-labourer in the field of natural history, I sincerely regret. As it would be tedious to enter into a minute consideration of the various misconceptions on the part of M. Virey, adverted to above, I shall particularize such only as directly militate against the theory I have endeavoured to establish relative to the ascent of aëronautic spiders in the atmosphere.

In narrating the fact observed by me in October 1826,—namely, that the tissues usually denominated gossamer-webs are formed at the surface of the earth, and are afterwards raised into the atmosphere by ascending currents occasioned by the rarefaction of the air contiguous to the ground when heated by the sun in serene weather,—M. Virey states that I witnessed the ascent of webs more than a hundred feet long, ("des traînées de plus de cent pieds de long de ces toiles,") and that I investigated the process employed by the *spiders* in fabricating "ces subtils *calicots*," as he facetiously terms them, in reference to the manufacturing district where my researches were made, "pour s'élever dans l'atmosphère et franchir au loin les espaces." Now, so far from ascribing the construction of these webs to spiders, in the instance referred to, I attribute their formation to the adhesion of the slender filaments of which they are composed on being brought into contact by the mechanical action of gentle airs. That the filaments are produced by spiders I am perfectly well aware, but I have no where asserted that these animals convert them into the webs termed gossamer; neither have I affirmed that they employ such webs to effect their aërial excursions, as M. Virey intimates: on the contrary, the accomplishment of their purpose is shown by me to depend upon ascending currents of rarefied air impingeing against the fine lines emitted from their spinners, which generally remain distinct throughout their entire length. The occurrence of spiders upon gossamer-webs I represent to be entirely accidental; and that the former are instrumental in promoting the ascent of the latter I positively deny: yet M. Virey professes to sày on my authority, that the animals aspire "à les faire envoler, en les fixant légèrement à l'extrémité d'un corps en pointe." My account of the height to which I have seen these webs raised in

in the air appears to have been mistaken by Mr. Virey for a statement of their length. This is the more surprising, as I have plainly indicated that they rarely exceed a *few feet* in longitudinal extent.

I forbear to offer any comments on the inaccuracy and confusion so conspicuous in M. Virey's survey of my opinions concerning the insufficiency of winds, evaporation, and electricity to occasion extensive and simultaneous ascents of gossamer-webs and spiders, and shall dismiss his review of my memoir with a few strictures upon the following passage, which clearly proves that not even my most careful expositions of the inferences I have deduced from exact experiments have escaped perversion. "Ces animaux" (spiders) "peuvent s'élever avec leurs tissus, et tantôt retomber selon le degré de gravité qui domine et les fait alors précipiter sur la terre. D'ailleurs, certaines particules d'air raréfie ne peuvent-elles pas se trouver comme renfermées dans le tissu gazeux de ces araignées et prendre à la manière des ballons un mouvement ascendant? L'auteur s'attache à développer l'idée de cette possibilité; il montre que des araignées peuvent expulser des fils à une certaine distance et les attacher par la matière gommo-visqueuse dont ils sont formés, à un lieu plus ou moins éloigné." In experimenting with a view to demonstrate the fact, that spiders do not raise themselves into the atmosphere by the exercise of any physical power with which they are endowed, I repeatedly separated from the spinners of individuals, when they were ascending, the fine lines which contributed to give them buoyancy. The result was conclusive. The animals were quickly precipitated to the ground, and the detached lines rose with increased velocity in consequence of being acted upon by a diminished gravitating force. How completely the true nature and object of this investigation have been misunderstood by M. Virey appears from the first sentence of the extract just given from his paper, upon which it would be superfluous to offer any comment. After inquiring, in the next place, whether the upward direction taken by gossamer-webs may not be ascribed to the rarefied air inclosed in them, M. Virey adds, that I have endeavoured to develop this idea; a piece of information which perplexes me not a little, as I am wholly unconscious of having made any such attempt. But the most remarkable circumstance remains to be considered. It is assumed by M. Virey, in opposition to the fact which I have uniformly and strenuously maintained,—a fact, let it be remembered, confirmed by direct experiments carefully conducted and accurately detailed,—that I have rendered it manifest that some spiders are capable of propelling their lines

lines to a distance. On what grounds this assumption, so contrary to the whole tenour of my experience, is founded, I have yet to learn.

I shall now proceed to examine the observations on the ascent of spiders communicated by M. Virey to the French Institute in June 1829, and published in the *Bulletin des Sciences Naturelles* for October in the same year, p. 130–134; availing myself of the opportunity thus afforded, to introduce several interesting particulars which have recently come to my knowledge. Having already rendered it sufficiently apparent that M. Virey does not comprehend my views relating to the ascent of gossamer-webs and spiders, any notice of the allusion made to them in the present article is unnecessary; I pass on therefore to the author's remarks on the power which he supposes spiders to possess of darting out their lines to a distance. "Je m'étois assuré déjà," he writes, "que jusqu'à la distance de deux pieds environ, une araignée savait lancer prestement un fil vers un point quelconque, l'y attacher, et s'enfuir soudain sur cette corde. Il faut que dans le nombre de leurs filières elles aient des tubes éjaculateurs, puisqu'elles lancent ces fils indépendamment d'autres sur lesquels elles s'avancent, et qu'elles émettent en même temps." This opinion, which has been entertained by so many eminent naturalists, may be refuted without difficulty. If spiders be placed on a twig fixed upright in a glazed earthen vessel with perpendicular sides, containing a sufficient quantity of water completely to encompass its base, they will be found totally incapable of escaping from their place of confinement in a still atmosphere; but when exposed to a current of air, or when blown upon with the breath, the case is otherwise, as many species may then be perceived to emit from their spinners a little of their viscous secretion, which being carried out in a line by the agitated air, becomes attached to some object in the vicinity, and affords them the means of regaining their liberty.

These facts were first established by me in 1826, in the manner here described; and in the summers of 1828 and 1829 I repeated the experiment with about thirty distinct species of spiders under every variety of circumstances which appeared likely to influence the result. My former conclusions, however, were most unequivocally confirmed, and I am confident in asserting that these animals have not the power of darting their lines even through the space of half an inch.

It is certain that spiders can open and close the orifices of their spinners at pleasure, and can allow their liquid gum to escape

escape so as to form one line or more of greater or less tenuity and strength, as may suit their convenience. When suspended by a vertical thread, for example, they frequently suffer another thread to be carried out horizontally, if blown upon in that direction; but that they have tubes so organized as to enable them forcibly to project their lines to a distance, as M. Virey conjectures, is quite inadmissible.

The curious fact, that all spiders possessing an apparatus for spinning are not endowed with the instinct to let out their lines when placed on a twig insulated by water and exposed to a current of air, I have proved by numerous experiments: and as this is the case with some of the more common species, as *Aranea domestica* and *Clubiona atrox*, I take this opportunity of calling the attention of investigators to the circumstance, which if unnoticed might occasion them some disappointment*.

M. Virey, who has experimented with various species of spiders, and especially with the young of *Epeira diadema*, informs us that "on doit faire ces observations dans une chambre close, où l'air très calme ne puisse recevoir aucune agitation." Being unable to detect the presence of any lines which he considered could contribute to the ascent of these animals, he concludes that the phænomenon must take place without their concurrence. "Réfléchissant," he remarks, "aux moyens par lesquels ces insectes gravissent dans l'air, une seule chose m'a paru la plus vraisemblable, c'est qu'à l'aide des huit pattes que l'animal peut faire vibrer avec agilité, *il nage dans l'air*. On conçoit que ces membres rapprochés, ramant quatre à quatre simultanément de chaque côté, frappent l'air comme des aîles, et peuvent fort bien enlever cet insecte d'ailleurs si léger. Ce procédé paraît le seul possible dans ce cas. D'ailleurs l'extrême rapidité, ou l'agilité incroyable de ces pattes en trépidation, comme la vibration des aîles chez les oiseaux ou les insectes diptères qui planent dans l'air, font qu'on ne peut pas toujours bien distinguer leur mouvement." And again, "Il est donc plus probable que ces petites araignées *volent avec leurs pattes*, que de supposer des effets électriques, ou l'agitation de l'air, ce que nous avons démontré faux par l'observation directe." In this bold but fanciful conjecture M. Virey has been anticipated by Dr. Lister, who, in treating upon his

* It would appear, however, from a notice respecting the habits of *Aranea domestica* published in the Zoological Journal, vol. i. p. 283, that, under certain circumstances, the instinct in question *is* manifested by that species of Spider.—Edit.

"araneus

"araneus subfuscus, minutissimis oculis è violâ purpurascentibus, tardipes, et gressu et figurâ cancro marino non adeò dissimilis," observes, "Certè egregius funambulus est, et mirificè filorum ejaculatione delectatur: neque solùm in aëre, utì superiores, vehitur; sed ipse etiam ascensum velificationémque molitur, pedibus sc. arctiùs ad se invicem applicitis sese quodammodo librat, cursum promovet regitque nihilo seciùs quàm si illi essent à naturâ concessæ alæ vel remorum ordines*." The extreme liability of air to be put in motion, and the exceeding levity of the lines of very small spiders, are facts which M. Virey, in the prosecution of his researches, has not attended to with sufficient minuteness. This is evinced by his supposing that in a close room the air could not be agitated to such a degree as to affect the results of his experiments. Have then, I would inquire, the temperature of the body, the motion of the limbs, and the act of respiration been altogether overlooked as causes of disturbance? It would appear that they have; no allusion whatever being made to them, in this respect, by M. Virey, who even recommends that the hand be passed before young spiders, when afloat, in order to determine whether they are supported by a line or not; a proceeding calculated to mislead the inquirer, if executed with haste, as it generally must be, by disturbing the air and occasioning a deflection of the line, which consequently might escape his observation.

These difficulties are entirely avoided by my mode of experimenting. The vessels containing the twigs on which the spiders are placed being generally locked up in a book-case, or put under bell-glasses, the tranquillity of the atmosphere immediately surrounding the prisoners is ensured without detriment to their physical powers. If then they are endowed with the capability of flying, what is to prevent them from exercising it when thus stimulated to exertion? Every facility is afforded them which they can enjoy when at liberty, except that of air in motion; yet they are never found to escape from the twigs by flight, notwithstanding their best endeavours to quit them are persisted in pertinaciously. I have tried this experiment with several hundred spiders of nearly thirty distinct species, including the *Epeira Diadema* in various stages of growth, and uniformly with the same success. In a calm atmosphere they are quite incapable of regaining their liberty, but if placed in a current of air, many species let out their lines with the utmost facility, along which, when attached to any object, they pass in security.

* De Araneis, p. 85.

To obviate such objections as might be urged against the use of glass or glazed earthen vessels by the advocates of the electrical hypothesis of the ascent of spiders, I have in many instances employed polished vessels of silver, tin, iron, &c. without producing the least difference in the results. I am not ignorant that these results have been represented as absolutely subversive of my views. Spiders, it is argued, are *compelled* to dart out their threads by the electrical excitation occasioned by currents of air, the phænomenon being considered inexplicable on any other principle whatever; but it is not found expedient to assign a reason for this sudden conversion of the power ascribed to these animals of propelling their lines to a distance, into a merely involuntary action entirely dependent upon air in motion. The fallacy of the foregoing supposition may be proved by placing spiders of the same species on copper rods insulated by water, and subjecting them to the influence of a stream of air so slight as scarcely to be perceptible. Under such circumstances, any electricity induced in the spiders by the feeble current must be carried away as speedily as it is excited, by so excellent a conductor as copper; nevertheless, the animals can emit or retain their liquid gum at pleasure, as lines may frequently be seen streaming from the papillæ of some individuals; while others, on the same rod, do not let out any, and may be instantly diverted from their purpose, should they make the attempt, by the most trifling causes of disturbance. It is manifest, therefore, that spiders do not fly, in the strict sense of the word; and that they are not raised into the atmosphere by the agency of electricity is equally evident; in short, not a doubt can be entertained by those whose minds are open to conviction, that their ascents are effected by means of upward currents of air impingeing against the lines which proceed from their papillæ.

The two species of aëronautic spiders, whose proceedings are detailed in the Linnean Society's Transactions, vol. xv. part ii., I have ascertained to be *Thomisus cristatus* and *Lycosa saccata*, both of which are distinctly mentioned as aëronautic spiders by Dr. Lister*. The first I have described as having two pair of eyes situated in the anterior part of the head and arranged thus, ·.. · ; the second as having three pair in front whose arrangement is thus represented by dots .:.:. . The species noticed in my paper as remarkable for the skill it displayed in spinning its way up the sides of a phial in which it was confined, and for having existed seventy-five days without food or *moisture*, was *T. cristatus; L. saccata* being neither so

* De Araneis, p. 79, 80, 85.

expert

expert in climbing, nor so tenacious of life under similar circumstances. I may remark, that in experiments instituted to decide how long spiders can live without food, the influence of season should not be disregarded. In winter, unless a high temperature be maintained by artificial means, the vital functions are performed with much less energy than they are in summer: of course the natural demand for sustenance, if it do not cease altogether, is greatly diminished in the former period, and the animals suffer little comparatively from abstinence.

Another spider of a diminutive size, frequently observed to take aërial excursions, is the *Drassus ater* of Latreille, which appears to be identical with the *Aranea obtectrix* of Bechstein.

Aëronautic spiders, properly so called, or those species which *instinctively* employ their lines to sail in the atmosphere, will probably be found almost exclusively among such as are active during the day and decidedly erratic. Numerous facts tend to corroborate this idea, the correctness or inaccuracy of which can only be determined by more extended observations.

XXIV. *On* Mengite, *a new Species of Mineral; on the Characters of* Aeschenite; *on* Sarcolite, *as distinct from* Analcime *and* Gmelinite; *with other Mineralogical Notices. By* H. J. Brooke, *Esq. F.R.S. L.S. & G.S.*

Ilmenite.

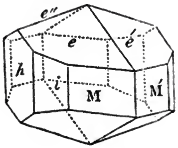
A MINERAL under this name is said to have been described in 1821 in Kastner's *Archiv.*, &c. No. 1. by Prof. Kupffer of Kasan. It was discovered by Mr. Mengé, near Lake Ilmen in Siberia, accompanied occasionally by a titanious iron-ore in modified rhomboids, of which a description and figure, but without measurements, were given by Mr. Levy in the Phil. Mag. and Annals, N.S. vol. i. p. 26.

Probably from having seen only the iron-ore, Prof. Rose of Berlin has stated that this was the ilmenite of Kupffer. The ilmenite is, however, a distinct substance, having for its primary form a *right rhombic prism* of 136° 30′, the terminal edge being to the lateral edge very nearly as 17 to 11. The colour is a more intense black than the rhomboids of titanious iron, and the surfaces of some of the crystals are perfect and brilliant. I have not observed any cleavage; the fracture is uneven to conchoidal with a vitreous lustre. Spec. grav. 5·43. Scratches glass slightly. The matrix is cleavelandite.

The crystals I have examined are generally small, length-

 ened

ened in the direction of the axis of the prism, and modified as in the accompanying figure.



Planes .... e, h, i.

Symbols .... $\overset{1}{B}$, $\overset{1}{G}$, $\overset{2}{G}$.

M,M′ = 136° 20′
M,e = 133 10
M,i = 151 36
M,h = 111 50
h,e = 104 44
e,e' = 150 32
e,e'' = 101 10

Titanious Iron.

The spec. grav. of the titanious iron is 4·74. Hardness much less than oligiste iron. The measurements of Mr. Levy's figure are

		Olig. Iron.	Axot. Iron.
p,a1...... =	122° 6′		
p,p, below	85 36	86° 10′	85° 59′

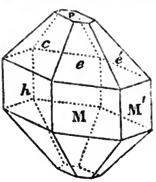
I give the axotomous iron on the authority of Mr. Haidinger's Mineralogy.

Mr. Levy's e3 does not occur on the crystals I have seen; but I find the planes of an obtuse rhomboid, produced by tangent planes on the terminal primary edges, and calling these b1, they give the following measurements:

a1,b1 = 141° 26′
b1,b'1 = 114 38

Aeschenite.

This mineral, which was also discovered and brought from Siberia by Mr. Mengé, and at first considered by him as gadolinite, has been analysed by Hartwell, and named by Berzelius; but as far as I can discover, not otherwise described than by a very imperfect notice given by Mr. Levy in the first volume of the present series of the Phil. Mag. p. 27. Some detached crystals I have lately obtained have enabled me to give the annexed figure and measurements, as taken from rough planes by the common goniometer.



M,M′ = 127°
M,h = 116 30′
M,e = 169 18
h,c = 143

Assuming the symbol of the plane e to be $\overset{1}{B}$, c is $\overset{1}{E}$, and a terminal

terminal edge of the prism is to a lateral edge nearly as 16 to 19. Its spec. grav. is 5·14. Hardness, between that of apatite and felspar. The colour of the fragments is brownish yellow, that of gadolinite being green.

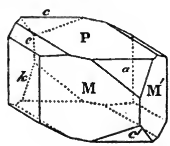
Mengite.

The mineral I am about to describe I have named after Mr. Mengé, who discovered it with the preceding ones near Miask, and whose mineralogical labours have probably not been exceeded by those of any of his contemporaries.

The *Mengite* occurs in imbedded crystals in masses of felspar and mica in a granitic rock. Its primary form is an oblique rhombic prism, whose terminal and lateral edges are to each other nearly in the ratio of 13 to 18. The planes are too dull for the reflective goniometer, and those of the larger crystals not sufficiently flat to afford very accurate measurement by the common goniometer.

The crystals of this substance present the accompanying figure, the measurements being nearly as follow:

P,M = 100°
M,M′ = 95 30′
P,a = 140 30
P,c = 125
P,e = 137 30



The laws of the planes.............. a, c, e, k,
are assumed to be...................... $\overset{1}{\mathrm{O}}$, $\overset{1}{\mathrm{A}}$, $\overset{2}{\mathrm{E}}$, $\overset{1}{\mathrm{G}}$,

The colour of the crystals is reddish brown. Hardness, between that of apatite and felspar. Spec. grav. 4·88. No regular cleavage. Fracture uneven, and the fractured surface dull. It has not been analysed, but from its specific gravity it is probably metallic. It is frequently attached to and penetrated by crystals of Aeschenite, and sometimes of zircon.

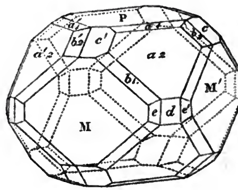
Sarcolite from Vesuvius.

This mineral appears to have been first observed by Dr. Thomson, who, as Haüy states, sent some fragments of crystals to him for examination, from which he inferred that the cube was its primary form, and conjectured that the mineral might be a variety of analcime. Hence red analcime has been called sarcolite, and the same name has also been given to red gmelinite. A specimen, with which I have been favoured by Mr. Heuland, and a fragment of a crystal which I have received

ceived from Dr. Donati, have enabled me to give the annexed figure and measurements of this substance; the primary form of which is a square prism, whose terminal and lateral edges are in the ratio of 62 to 55 very nearly.

The planes	$a1$,	$a2$,	$b1$,	$b2$,	c,	d,	e,
are produced by the laws	$\overset{3}{A}$,	$\overset{1}{A}$,	${}_3A_3$,	${}_0A_{\frac{1}{3}}$,	$\overset{1}{B}$,	$\overset{1}{G}$,	$\overset{2}{G}$.

P,$a1$	=	157° 19′
P,$a2$	=	128 33
P,d	=	90
P,c	=	138 25
M,$b1$	=	153 20
M,$a2$	=	123 34
M,$b2$	=	102 28
M,e	=	153 26
M,d	=	135

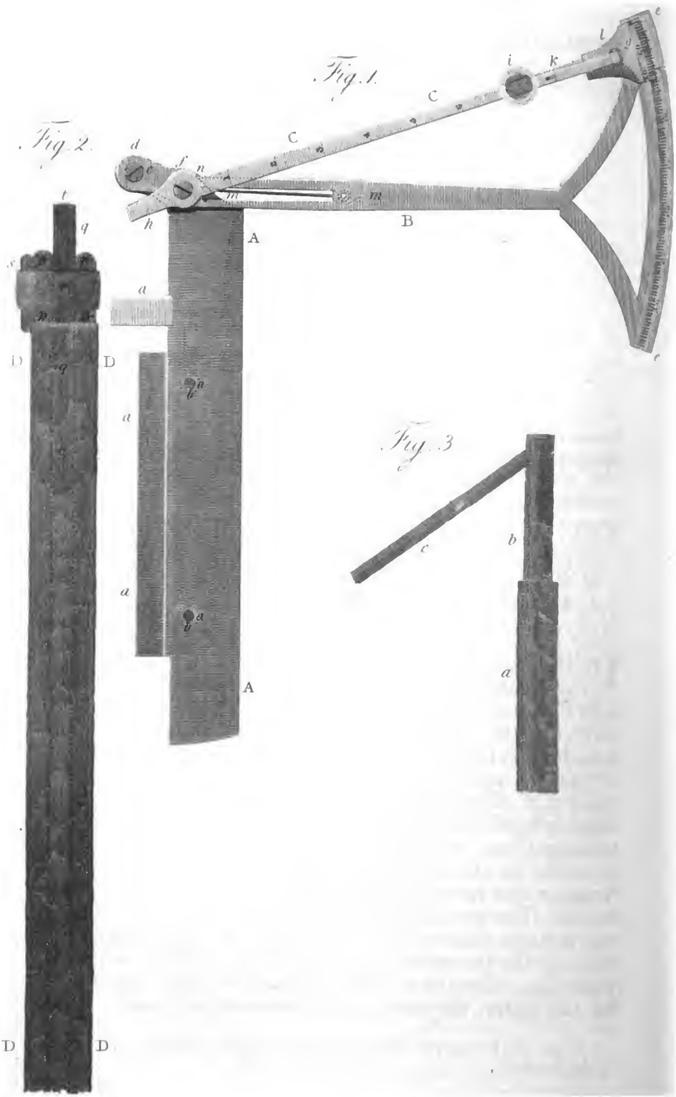


The plane $b2$, as the symbol denotes, and as appears in the figure, occurs singly on each angle, instead of being accompanied by a corresponding plane on the other side of c.

Wollastonite from Vesuvius.

This is the same mineral as was, according to the ticket accompanying specimens from Vesuvius, formerly named *Zurlonite* or *Zurlite*. The crystals are generally very imperfect, and the surfaces very dull; a specimen, however, in the possession of Dr. Somerville presented some bright and well-defined crystals, an examination of some of which has enabled me to give the annexed figure and measurements. The primary form is an oblique rhombic prism. There is one cleavage parallel to P, and three others parallel to the three planes $a3$, h, $c2$; that parallel to h is the brightest, and I have supposed it parallel to the edge of the prism, and have thus assumed a different cleavage angle from that usually assigned to tabular spar. I have, however, found a cleavage corresponding to the plane h, in specimens from the Bannat, in which the cleavages usually observed and hitherto quoted are those parallel to P and $c2$. The ratio of the terminal and lateral edges of the prism which I have taken as the primary is very nearly as 25 to 40, and the obtuse angle of the terminal plane is 91°·56′. The planes are more than usually perfect, several of the measured angles having agreed exactly with those given by calculation.

Planes



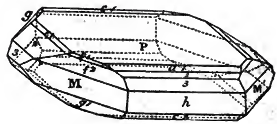
Mr. Daniell's Register-Pyrometer.

J. Porter Sc.

Planes *a*1, *a*2, *a*3, *c*1, *c*2, *e*1, *e*2, *e*3, *f*1, *f*2, *g*, *h*.

Symbols $\overset{5}{O}$, $\overset{3}{O}$, $\overset{1}{O}$, $\overset{3}{A}$, $\overset{1}{A}$, $\overset{3}{E}$, $\overset{2}{E}$, $\overset{1}{E}$, $\overset{1}{D}$, $\overset{\frac{1}{2}}{D}$, $\overset{\frac{4}{2}}{B}$, $\overset{1}{H}$.

P,*a*1	=	159° 30′
P,*a*2	=	150 23
P,*a*3	=	129 42
P,*h*	=	110 12
P,*f*1	=	132 55
P,*f*2	=	120 42
P,M	=	104 48
P,*g*′	=	86 8
P,*e*1	=	145 7
P,*e*2	=	133 43
P,*e*3	=	115 33
P,*g*	=	93 52
M,M′	=	95 38



Some of the crystals are hemitrope, the plane of revolution being parallel to the terminal plane.

XXV. *On a new Register-Pyrometer, for Measuring the Expansions of Solids, and determining the higher Degrees of Temperature upon the common Thermometric Scale.* By J. Frederic Daniell, *Esq. F.R.S.**

[With a Plate.]

IN the year 1821 I published in the Journal of the Royal Institution† an account of a new pyrometer, and the results of some experiments with it, which were the means of correcting the highly erroneous notions which had, up to that time, been generally entertained of the degrees of temperature beyond the boiling point of mercury. The instrument was capable of affording correct determinations, connected in an unexceptionable manner with the scale of the mercurial thermometer; but, although applicable to scientific investigation in careful hands, it could be inserted only into experimental furnaces of a particular construction, which greatly limited its use. The great desideratum still remained of a pyrometer, which might universally be applied to the higher degrees of heat, as the thermometer has long been to the lower; and which, in addition to its use in delicate researches, might effect for the potter, the smelter, the enameler and others, in the

* From the Philosophical Transactions for 1830: Part II., and revised by the Author. † Vol. xi. p. 309.

routine

routine of their business, what the latter daily performs for the brewer, the distiller, the sugar-refiner, and the chemist.

I shall now have the honour of laying before the Royal Society a description of a contrivance which, I trust, will be found to answer all the desired purposes; and which, while simple enough to be intrusted to the hands of common workmen in every variety of fire-place, I hope to prove, by the results of my experiments, to be sufficiently delicate to extend considerably our knowledge of the expansion of metals, upon which so much labour has been bestowed by some of the first philosophers.

I was not aware, at the time when I wrote the account above referred to, that the subject had been previously investigated by M. Guyton de Morveau, and that he had proposed to apply the expansion of platinum as a measure of high temperature, and more particularly to the purpose of connecting the indications of Wedgwood's pyrometer with the mercurial scale and verifying its regularity. I have since carefully studied his laborious papers in the *Annales de Chimie**, and the *Mémoires de l'Institut*†, which appear to have been but very little known in this country; and previously to entering upon the more particular object of the present paper, I must claim indulgence for a few remarks upon the general state of the inquiry at the time when its pursuit was abandoned by that able philosopher.

M. Guyton's pyrometer consisted of a small bar or plate of platinum 45 millimetres (1.77 inch) long, 5 millimetres (about 0.2 inch) broad, and 2 millimetres (about 0.08 inch) thick, placed in a groove formed in a piece of highly baked porcelain. One extremity of this bar rested upon the solid end, which terminated the groove, and the other pressed upon the short arm of a bent lever, the longer arm of which terminated in a point and moved on a pivot over the graduated arc of a circle; indicating by its motion any lengthening of the bar by increase of temperature. The short arm of the lever was 2.5 millimetres and the long arm 50 millimetres in length, and the latter carried a nonius by which the tenths of a degree might be read off. The whole was constructed of platinum; and a plate of the same metal was made to press, in the manner of a spring, upon the extremity of the index, to prevent any displacement when withdrawing it from the fire. The description of this instrument in the first Essay, published in the year 1803, was not accompanied by any explanatory figure; and the notice in the *Annales* terminates by announcing that the

* Tome xlvi. p. 276.

† 1808, Second Semestre, tome ix.—1811, ibid. tome xii.

inventor

inventor had at that time only begun "a series of experiments to determine its march, to compare it with the pyrometer pieces of Wedgwood, and to ascertain the degree of confidence which might be placed in the indications of the latter." The second Essay did not appear till the year 1808, and in it M. Guyton observes that "many persons had expressed a wish to be made acquainted with the improvements which he had made in the instrument since its first construction; and that he had determined in consequence to give a fresh description of it accompanied by drawings, which might enable artists who undertook its construction to render it comparable. He, however, thought it right to give a previous account of the labours of others in this branch of science, and to remove certain errors which had prevailed up to that time concerning the pyrometer then most in use (Wedgwood's), and which might possibly prove most commodious, and consequently most useful, if once the degree of exactitude could be determined of which it was susceptible." The remainder of the paper is taken up with an account of the most accurate experiments upon the expansion of the metals from the time of Newton.

The third and last Essay was delayed till the year 1811; and in it no further description of the platinum pyrometer is to be found; but a laborious comparison,

1st, of the indications of the platinum pyrometer with those of the mercurial themometer;

2nd, of the same pyrometer with that of Wedgwood; and

3rd, of the degrees determined by these instruments with those previously known of the expansion, ebullition and fusion of various substances; in a range of temperature comprising the highest degrees of the thermometric scale and the lowest of Wedgwood's.

Now it is very remarkable that all M. Guyton's efforts in this paper are directed to the valuation of the degrees determined by Mr. Wedgwood's clay pieces; but that he carries the comparison of the platinum pyrometer by actual experiment no higher than the melting point of antimony. He clearly establishes a great error in Mr. Wedgwood's original estimation of his degrees to that point; and, by calculation upon this basis, continues the correction to the melting point of iron, "en admettant toujours une progression uniforme jusque dans les plus hautes températures." The experimental comparison was obviously stopped by some practical difficulty at higher temperatures; and it is easy to perceive in what this must have consisted. Platinum at a red heat becomes very soft and ductile; and the lever against which the pyrometric bar

bar pressed, being of such very slender dimensions, would obviously be liable to bend, and thus frustrate the experiment: in addition to which, I can speak from my own experience, the platinum spring plate and the centre pin would be liable to a change of texture, which would impede the motion of the lever, and it would finally become welded to the index; for a very moderate pressure at a high temperature would produce this effect.

The conclusion, indeed, of these Essays seems to admit that the author did not expect that the platinum pyrometer could ever come into general use: "enfin, ces corrections ne peuvent manquer d'ajouter à l'utilité du pyromètre d'argile, soit dans les travaux chimiques, soit dans les arts; quand même le pyromètre de platine, plus exact mais moins usuel, serait réservé pour en assurer la marche, et pour servir à des recherches plus importantes."

M. Guyton, however, although he abundantly proves the incorrectness of Mr. Wedgwood's estimate of the higher degrees of temperature, is very far indeed from establishing the point at which he so earnestly laboured, namely, the regularity of the contraction of the clay pieces; or from substituting a more correct value of the degrees throughout the whole range of the gauge than the one which he so completely overturned. His comparative experiments with the platinum pyrometer, at the boiling points of mercury and linseed oil, and the melting point of antimony, led him to reduce the equivalent of each degree from 130° Fahr. to 62°·5. The zero point of the clay pyrometer was thus carried back to 517° instead of 1077°; but it seems to have escaped his notice that this zero point was declared to be a red heat visible in the daylight,—a description which cannot be mistaken, and which clearly could not be below the temperature of boiling oil, melting lead, or boiling mercury; all of which are, however, placed above it in M. Guyton's table. M. Guyton also places the melting point of silver at the 22nd degree of Mr. Wedgwood's scale instead of the 28th, which was, according to his own determination, a correction first suggested by Sir James Hall in the 9th volume of Nicholson's Journal. Taking the value of each degree at 62°·5 Fahr., it fixes this point at 1892° Fahr., which agrees very nearly with my own experiment in the paper before alluded to; but continuing the calculation up to the melting point of iron, upon the supposition of an uniform progression, the 130th degree corresponds with 8696° Fahr., which, although only about half the amount 17977° assigned by Mr. Wedgwood, is very far removed from the result of my calculation 3479°.

Never-

Nevertheless, it is a curious fact, that M. Guyton's Essay contains proof that his determination is erroneous, and that mine is a near approximation to the truth. As a collateral means of verifying the indications of instruments intended to measure high degrees of temperature, he refers to the calorimeter as capable of affording the necessary data by a calculation from the amount of heat communicated to known quantities of ice or water by bodies in a state of incandescence; and he quotes the very exact experiments of MM. Clement and Desormes, who had in this manner determined the following points:

	By the Liquefaction of Ice. Fahr.		By the Heat communicated to Water. Fahr.
Temperature of soft iron melted ...	3988°	......	3902°
Cast iron just on the point of melting	3164	......	——
Red hot iron	2732	......	——
White hot ditto......................	3282	......	——
Iron just ceasing to be luminous in day-light	——	......	1272
Melted copper	——	......	2294

My own determinations of the melting point of cast iron, 3479°, of that of copper, 2548°, and of a red heat, about 1000°, agree very closely and satisfactorily with these results, with which I was unacquainted at the time of my experiments. M. Guyton's remark upon the latter is: "Il suffit de jeter un coup-d'œil sur les résultats, pour recueillir de nouvelles preuves univoques de la nécessité de réduire les valeurs données par Wedgwood aux degrés de son pyromètre. Mais je ne crains pas de dire que ces réductions sont ici portées trop loin, ainsi qu'on peut en juger en les rapprochant de celles auxquelles j'ai été conduit par l'ensemble des expériences rapportées dans cet essai. Ce n'est pas que je veuille répandre des doutes sur l'exactitude des observations dont je dois la communication aux deux habiles chimistes ci-dessus cités; mais il est aisé de faire voir que la différence des résultats est due, pour la plus grande partie, à la différence des procédés; de sorte que les évaluations qu'ils ont données aux degrés de l'échelle de Wedgwood, peuvent, en dernière analyse, et en prenant les termes moyens dans la latitude que comportent des opérations aussi délicates, servit plutôt à confirmer qu'à détruire le système de correction que j'ai établi."

It is worthy of observation, that had the degrees of Wedgwood's pyrometer been valued from this determination of the fusing point of iron, the result would have better corresponded with the whole series of phænomena. Instead of 130° Fahr. as fixed by the inventor, or 62°·5 as corrected by M. Guyton,

 they

they would have been estimated at about 20° Fahr.; and taking Mr. Wedgwood's original determination of the fusing point of silver at 28° of his scale and the zero point at 1077°, the former would come out about 1650°. By raising the zero point a little, (and it is much more probable that the temperature of a red heat fully visible in the day-light is above 1077° than below it,) we arrive at something like an approximation to the truth. These wide discrepancies, and the practical disuse of both Mr. Wedgwood's and M. Guyton's pyrometers for a long time past, prove the expediency of further investigating a subject of so much interest and importance.

The pyrometer, which I shall now proceed to submit to the judgement of the Society, consists of two distinct parts, which I shall designate as the Register and the Scale.

The first is a solid bar of black-lead earthenware, eight inches long, seven-tenths of an inch wide, and of the same thickness, cut out of a common black-lead crucible. In this a hole is drilled three-tenths of an inch in diameter, and 7½ inches deep. At the upper end of this bar and on one of its sides about six-tenths of an inch in length of its substance is cut away to the depth of half the diameter of the bore. When a bar of any metal 6½ inches long is dropped into this cavity, it rests against its solid end; and a cylindrical piece of porcelain about 1½ inch long, which I shall call the index, is placed upon the top of it, which projecting into and beyond the open part, is firmly confined to its place by a ring, or strap of platinum; which passing round the black-lead bar and over the piece of porcelain, is made to press upon the latter with any required degree of tension by means of a small wedge of porcelain inserted between the bar and the strap on the side of the former. It is obvious that when such an arrangement is exposed to a high temperature, the metallic bar will force the index forward to the amount of the excess of its expansion over that of the black-lead, and that when again cooled, it will be left at the point of greatest elongation. It may also be observed, that the exact indication of this amount is not in the slightest degree interfered with by any permanent contraction which the black-lead may undergo at high degrees of heat; as any such contraction will take place at the moment of the greatest expansion of the metal, and the index will still mark its point of furthest extension upon this contracted basis.

The problem now consists in the accurate measurement of the distance which the index has been thrust forward from its original position; and although the amount can in any case be but small, there is no reason why it may not be determined with the same precision as is now commonly attained in similar

quantities

quantities in astronomical and geodetical operations. For this purpose the scale is constructed of two rules of brass, accurately joined together at a right angle by their edges, and fitting square upon two sides of the black-lead bar, and of about half its length. At one end of this double rule a small plate of brass projects at a right angle, which plate, when the two sides of the former are applied to the two sides of the register, is brought down upon the shoulder formed by the notch cut away at its upper end, and the whole may be thus firmly adjusted to the black-lead bar by three planes of contact.

On the outside of this frame another brass rule is firmly screwed down, which projecting beyond it, and bending a little so as to bring its end opposite to the cavity in the black-lead bar when applied to it, supports a moveable arm exactly 5½ inches long, turning at its fixed extremity upon a centre, and at its other carrying an arc of a circle, accurately divided into degrees and thirds of a degree, whose radius is exactly 5 inches. At the centre of this circle upon the arm, and of course at the distance of half an inch from the centre of motion, another lighter arm is made to turn, one end of which, being the exact radius of the circular arc, carries a nonius with it, which moves upon the face of the arc and subdivides the former graduation into minutes. The other end crosses the centre; and at the exact distance of one-tenth of the radius, or the distance between the two centres of motion, terminates in an obtuse steel point turned inwards at a right angle. These graduations and distances are laid down with the greatest precision by Mr. Troughton's dividing engine. This part of the apparatus may be regarded as a pair of proportional compasses attached to the end of the brass rule and frame, whose longer legs carrying the arc and nonius are to its shorter as ten to one; and the opening of the latter being regarded as a chord of a small circle, is magnified in the same proportion by the former, and measured upon the scale. A small steel spring let into the larger arm is made to press upon the smaller, so as to adjust the nonius to the commencement of the graduation; and when forced back it tends to restore it to its original position.

The annexed figures, in which all the parts are drawn of their real dimensions*, will assist the comprehension of the preceding description. Plate II. fig. 1. represents the scale. A A is the principal brass rule, upon the under side of which the frame *a a a a a a'* is adjusted by the screws *b b*, and which

* In our plate the pyrometer is represented about one third less than its real dimensions.—Edit.

supports

supports upon its bent extremity *c*, the arm B moving upon the centre *d*, and terminating in the arc of the circle *e e*.

C C is the lighter arm moving upon the centre *f* upon the arm B, and carrying at one end the nonius *g*, and at the other the steel point *h*, the distance of which from the centre *f* is exactly half an inch or one-tenth of the radius *f g*, and equal to the distance of the two centres *f d*. *i* is a small lens represented as lying down, but which may be raised by the centres *k* and *l* perpendicularly above the nonius to facilitate the reading. *m m* is the steel spring, which being fixed in a cavity cut out of the arm B, presses upon a small pin *n* on the arm C, and throws the radius back to the commencement of the arc.

Fig. 2. represents the register. D D D D is the black-lead bar, with its cavity *o o*. At *p p p p* it is cut away to the depth of half the bore. *q q* is the porcelain index, which is placed upon the top of the metallic bar, and confined to its place by the pressure of the platinum strap *r* acting by the force of the small porcelain wedge *s*.

When an observation is to be made, the metallic bar is placed in the cavity of the register, the index is to be pressed down upon it and firmly fixed in its place by the platinum strap and porcelain wedge. The scale is then to be applied by carefully adjusting the brass rules to the sides of the black-lead bar, and fixing it by pressing the cross piece (a') upon the shoulder: holding the whole together steadily in the left hand, the moveable arm should be so placed that the steel point (*h*) of the other leg of the compasses may rest upon the edge of the porcelain index, against which it will be pressed with some force by the spring: then moving the arm gently forward with the right hand, the point will slide along the end of the index till it drops into a small cavity (*t*) formed for its reception, and which exactly coincides with the axis of the metallic bar in the register, and the centre of motion of the compasses on the brass rule. The minute of the degree must then be noted, which the nonius indicates upon the arc. A similar observation must be made after the register has been exposed to an increased temperature and again cooled; and the number of degrees or minutes which the nonius will then mark will, by a simple calculation from the known length of the radii and angle, give the length of the chord comprised between the original position of the compasses and the point to which they have moved, or the distance which the index has been forced forward. Such an operation appears complex in the description, but is in fact extremely simple after a little practice, and does not require more than a few seconds for its performance. The scale of this pyrometer being completely detached

detached from the part which is exposed to the fire, obviates one important objection which has always been made to other contrivances of the same nature, from the uncertain degree of heat and expansion to which they are liable; while the simplicity of that part of the arrangement which alone is subjected to great heats, renders it little liable to injury; and together with the cheapness of the materials of which it is constructed, occasions but a very trifling expense for replacing it when injured.

The calculation of the absolute expansion of the bar indicated by the scale may be performed as follows:—As radius to double the sine of half the arc read off, and found in a table of natural sines, so will the radius B be to the chord of the same arc; and this divided by ten (the radius of B being ten times the length of the radius *f h*) will give the length required. Suppose the arc read off upon the scale to be 4°,

$$\text{then } \overset{\text{Radius.}}{1\cdot0000000} : \overset{\text{Sine of } 2^{\circ}.}{0348995} \times 2 :: \overset{\text{Inches.}}{5} : \overset{\text{Inch.}}{\cdot3489950} \div 10 = \cdot0348995.$$

Now in working out this proportion it will be observed, that the multiplication by 2 and by 5 being both constant may, in conjunction with the division by 1·0, be omitted; and leaving out also the final division by 10, the case resolves itself into seeking the sine of half the arc, read off upon the scale, in a table of natural sines, and reading it as the decimal of an inch.

Moreover, the chords of small arcs are so nearly proportional to their arcs that, the number of degrees measured upon the scale never exceeding 10, they may be considered without sensible error as denoting equal increments of expansion. The following short Table of the value of a degree, and minutes of a degree, may therefore be useful in practice.

TABLE I.

°	′		Inch.
1	0	=	·00872
0	30	=	·00436
0	20	=	·00290
0	15	=	·00218
0	10	=	·00145
0	5	=	·00072
0	2	=	·00029
0	1	=	·00014

The chord of ten degrees derived from this Table by multiplying ·00872 by 10 would therefore be ·0872, whereas it is more accurately ·0871; but the difference being only $\frac{1}{10,000}$dth of an inch may, in most cases, be disregarded.

[To be continued.]

XXVI. *On*

XXVI. *On the Theory of the Compressibility of the Matter composing the Nucleus of the Earth, as confirmed by what is known of the Ellipticities of the Planets.* By the Rev. J. CHALLIS, *Fellow of the Cambridge Philosophical Society**.

DR. YOUNG suggested that the increase of density towards the centre of the earth, might be owing to the compressibility of the material of which it is composed. Laplace, adopting the suggestion, obtained, in an addition to a Memoir on the Figure of the Earth (*Mém. Acad. Scien.* An. 1818) the law of the increase of density in proceeding from the surface to the centre, on the suppositions that the relation between the pressure (p) and density (ϱ) is expressed by $p = k^2\left(\frac{\varrho^2}{\delta^2} - 1\right)$, ($\delta$ being the density at the surface), and that the chemical composition of the nucleus of the earth is the same throughout. He found that on these suppositions the requisite degree of compressibility, and the proportion of the density at the surface to the mean density, were not by any means at variance with what we know on these points by experience. The cause assigned in this theory for the increase of density towards the centre, and the relation between p and ϱ, are of so simple a nature, that I have been induced to inquire how far the theory is confirmed by what is known of the ellipticities of the planets.

I here repeat the investigation of Laplace, modified for the purposes I have in view. Suppose the mass of the planet to be spherical. If r be any distance from its centre, and $\varrho = \phi(r)$, the attraction on a particle of the mass at a distance R from the centre is $\frac{\int 4\pi r^2 \phi(r)\, dr}{R^2}$, the integral being taken from $r = 0$ to $r = R$. To express this force in the manner in which terrestrial gravity is usually expressed, let M = the earth's mass, a = its radius, and $g = 32\frac{1}{6}$ feet, the measure of the accelerative force of gravity at the earth's surface: then $\frac{g a^2}{M} \cdot \frac{\int 4\pi r^2 \phi(r)\, dr}{R^2}$, is the force expressed as required. Suppose that $\int r^2 dr\, \phi(r) = \psi(r)$. Then attraction

$$(A) = \frac{4\pi g a^2}{M} \times \frac{\psi(R) - \psi(0)}{R^2}. \quad \text{Now } -dp = A\varrho\, dR;$$

$$\text{and } -dp = -\frac{2k^2}{\delta^2}\varrho\, d\varrho.$$

$$\text{Therefore } \frac{4\pi g a^2 \varrho}{M} \cdot \frac{\psi(R) - \psi(0)}{R^2} = -\frac{2k^2 \varrho\, d\varrho}{\delta^2\, dR}.$$

* Communicated by the Author.

Hence

Hence $\psi(\mathrm{R}) - \psi(0) + \frac{2k^2 \mathrm{M}}{4\pi g a^2 \delta^2} \cdot \frac{\mathrm{R}^2 d\varrho}{d\mathrm{R}} = 0$; and differentiating, $\mathrm{R}^2 \phi(\mathrm{R}) + \frac{k^2 \mathrm{M}}{2\pi g a^2 \delta^2}\left(\frac{\mathrm{R}^2 d^2\varrho}{d\mathrm{R}^2} + \frac{2\mathrm{R}\, d}{d\mathrm{R}}\right) = 0$.

Hence finally, $\frac{d^2 \mathrm{R}\varrho}{d\mathrm{R}^2} + q^2 \mathrm{R}\varrho = 0$, q^2 being $\frac{2\pi g a^2 \delta^2}{k^2 \mathrm{M}}$.

The integral of this gives $\frac{\varrho}{\varDelta} = \frac{\sin q\mathrm{R}}{q\mathrm{R}}$, $\varDelta$ being the value of ϱ at the centre. Legendre has calculated (*Mém. Acad. Scien.* An. 1789), that according to this law of density, if $c =$ the earth's radius, and $qc = \frac{2\pi}{3}$, the ellipticity would be $\frac{1}{269}$; and if $qc = \pi$, the ellipticity would be $\frac{1}{379}$. But it is plain from the nature of solid substances that qc cannot be so great as π, for then ϱ would be $= 0$. In proportion as the value of $q\mathrm{R}$ is near to π, the density is small, and decreases rapidly as R increases: and because solid substances do not admit of this rapid change of density, and at their surfaces possess a certain limited density, therefore it is probable that we shall not be far wrong by assuming $qc = \frac{5\pi}{6}$. In fact, this value gives an ellipticity of $\frac{1}{306}$ to the earth, which is very near the experimental determination.

If now we calculate the attractive force of Jupiter at his equator, by means of the period of his fourth satellite, and the law of the inverse square of the distance, we shall find that the ratio of the centrifugal force at his equator to this force is $\frac{1}{11{\cdot}95}$. But because the law of the inverse square does not accurately hold by reason of the planet's spheroidal shape, this value requires a correction. When the correction has been made according to the formula given in the *Méc. Céleste*, liv. iii. art. 35, the ratio becomes $\frac{1}{12{\cdot}348}$. The ratio of the centrifugal force to gravity at the equator of Saturn, calculated from the period (79·33 days) and mean distance (64·36 equatorial radii) of the extreme satellite, and from the time ($10^{\mathrm{h}}\ 15'$) of Saturn's rotation, will be found to be $\frac{1}{7{\cdot}76}$.

Correcting as before, the ratio becomes $\frac{1}{8}$ nearly. Calcu-

lating now the ellipticities of Jupiter and Saturn by Clairaut's Theorem, on the supposition that $q\,c = \frac{5\,\pi}{6}$, we shall find for Jupiter $\frac{1}{13\cdot 1}$, and for Saturn $\frac{1}{8\cdot 4}$. The measurements of Professor Struve determine the ellipticity of Jupiter to be $\frac{1}{13\cdot 71}$, and Herschel obtained $\frac{1}{11}$ for the ellipticity of Saturn. It must be observed that the above calculations take into account only the first power of the ellipticity, and therefore cannot be very accurate with respect to Jupiter and Saturn, the ellipticities of which are not very small. We may, however, affirm that the ellipticity of Jupiter accords very well with the theory we are considering. That of Saturn is considerably less than what the theory gives. Herschel remarked an anomaly in the shape of this planet, which, however, subsequent observations have not yet confirmed; viz. that the equatorial diameter was not so large as a diameter about midway between the equatorial and the polar. It would seem, if this be true, that some cause has operated to compress the parts about the equator. The same cause would account for an ellipticity less than what our theory requires. Possibly the rings may have something to do with this.

Venus, Mercury, and the Sun, in as much as they possess no ellipticity discoverable by instruments, do not contradict the theory. But Mars forms an exception. Its ellipticity is ascertained by observation to be $\frac{1}{16}$; whereas the ratio of the centrifugal force to gravity at its equator, which ratio differs little from the ellipticity that the theory gives, will be found to be $\frac{1}{254}$, by taking ·1386 for the ratio of its mass to that of the earth, ·1294 the ratio of the volumes, and 24·67 hours the period of its rotation. The great ellipticity of this planet, considering the time of its rotation and its small size, is remarkable, and seems not to be in accordance with Clairaut's Theorem, unless we suppose the gravity of the planet to diminish in passing from the equator to the pole.

If the cause assigned in this theory be sufficient to account for the increase of the density of a planet towards its centre, then on the supposition that the nuclei of the planets are all as to chemical composition homogeneous, and are similarly constituted, though of different mean densities, the equation $q\,c = \frac{5\,\pi}{6}$ ought to be nearly true for all, since it is nearly

true

true for the earth. Let us see what will follow from supposing this equation to be generally true. If the mean density of the earth be d, then $M = \frac{4\pi a^3 d}{3}$, and $q^2 = \frac{3 g \delta^2}{2 k^2 a d}$. Now the velocity (V) of propagation in a medium in which $p = k^2\left(\frac{\varrho^2}{\delta^2} - 1\right)$, is $\frac{k}{\delta}\sqrt{2\delta}$, where the density is δ. This may be shown by a separate consideration of this particular case, or be inferred from a general proposition respecting the velocity of propagation, which I gave in the Phil. Mag. and Annals of Philosophy, for May 1830*, where it was proved that if $p + C = a^2 \varrho^{1+n}$, V may be found from the equation $V^2 - a^2 \varrho^n (1+n) = 0$. We have then, $\frac{V^2}{2\delta} = \frac{k^2}{\delta^2}$; $q^2 = \frac{3 g \delta}{V^2 a d}$; and $q^2 c^2$, or $\frac{25\pi^2}{36} = \frac{3 g c^2 \delta}{V^2 a d}$. Now Laplace has calculated that according to this theory the ratio of the mean density to the density at the surface of the earth is 2·42; and according to our supposition the same ratio holds true for the planets.

Therefore if D = the mean density of the planet, $\frac{g c^2 D}{V^2 a d} = \frac{25\pi^2 \times 2{\cdot}42}{3 \times 36} = 5\tfrac{1}{2}$ nearly. Hence if v = the velocity of propagation in the material which composes the nucleus of the earth, $\frac{g a}{v^2} = 5\tfrac{1}{2}$, or $v = \sqrt{\frac{2 g a}{11}}$: v being calculated from this, is found to be 10·13 times the velocity of propagation in air:—a result far from being improbable. Generally, $\frac{g c^2 D}{V^2 a d} = \frac{g a}{v^2}$. Whence $\frac{V}{v} = \frac{c}{a}\sqrt{\frac{D}{d}} = \sqrt{\frac{c^3 D}{a^3 d} \cdot \frac{a}{c}}$ $= \sqrt{\frac{m a}{M c}}$, m being the mass of the planet, M of the earth.

* I have there shown that, *the velocity of uniform propagation* $= \frac{\textit{the velocity in the medium}}{\textit{Naperian log. of the density}}$. This equation bears the same relation to the propagation of motion, as the equation, *uniform velocity* $= \frac{\textit{space}}{\textit{time}}$, to motion itself, and will not seem unimportant to those who consider that the one phænomenon is nearly as frequent as the other. As the proposition is quite new, and in some degree contradicts the received mode of determining the velocity of propagation, it is not likely to meet with immediate attention: I have therefore adverted to it here.

 The

The following values of $\frac{V}{v}$, are calculated from the masses of the bodies of our system, as given by Laplace. For the Sun, 54·31; Jupiter, 5·33; Saturn, 3·06; Uranus, 1·97; the Earth, 1; Venus, ·981; Mercury, ·651; Mars, ·5; the Moon, ·23. It is observable that the order of magnitude of these quantities is the same as the order of the bodies arranged according to their masses. It follows, therefore, from our theory, that because in the greater masses the velocity of propagation is greater, the materials of which they are composed possess greater elastic force. This may be owing, not to any difference in the constituent elements, but to a greater degree of proper caloric, or of the force, whatever it be called, by which the constituent atoms are held in their positions relatively to each other. For it is not unreasonable to suppose that the proper underived caloric of any mass, such as we know exists in the earth and forms the principal part of its caloric, is some function of the mass, and is specifically greater as the mass is greater. The Sun, which is the largest, is the hottest body of our system. According to this view, if there be granite at the surface of the moon, it will be more compressible than the granite of the earth; it will possess both a density and a compressibility depending on the mean density and compressibility of the moon's nucleus.

The theory we have been considering requires us to believe that the interior of a planet is solid, and not fluid. On the supposition of fluidity, it would be difficult to account for the contradiction presented by Mars to Clairaut's Theorem. May we not conjecture, that this planet is hollow about its centre, or in the direction of its axis? Generally speaking, the least bodies of the solar system are the densest, if we set aside the satellites, which seem to partake of the density of their primaries. But Mars is not so dense as Mercury, Venus, or the Earth. This fact favours in some degree the conjecture.

Assuming the truth of our theory, we may readily conceive that any change in the state of the internal heat of the earth, would give rise to great changes *at its surface*, and perhaps produce effects like those exhibited by geological phænomena.

Upon the whole, a review of the planets seems to favour the idea, that any increase of density towards their centres, is owing either wholly, or at least in part, to the compressibility of the matter of which their nuclei are composed.

Papworth, St. Everard, Aug. 10, 1831.

XXVII. *Analysis*

XXVII. *Analysis of some Salts of Mercury.* *By* R. Phillips, F.R.S. L. & E. &c.

WHEN two parts of mercury are heated, for a short time, with three of sulphuric acid, some protosulphate of mercury is formed; but if the heat be continued, the mercury is converted almost entirely into bipersulphate, even before the evolution of sulphurous acid gas ceases, or the whole of the metal is dissolved. When this bipersulphate of mercury is put into water it is decomposed, and a yellow precipitate, formerly called turpeth mineral, is thrown down.

This salt was, I believe, first minutely examined by Fourcroy (*Annales de Chimie*, tom. x. p. 109.), and according to his analysis, it consists of

Sulphuric acid	10
Mercury	76
Oxygen	11
Water	3
	100

MM. Braamcamp and Sequeira (*Ann. de Chimie*, tom. liv. p. 123.) give as the result of their analysis,

Sulphuric acid	15
Peroxide of mercury	84·7
Loss, attributed to moisture	00·3
	100·0

Dr. Thomson (*System*, vol. ii. p. 660.) observes, that supposing it to be a compound of 1 atom acid + 1 atom peroxide, its constituents will be

Sulphuric acid	15·62
Peroxide of mercury	84·38
	100·00

In his *Attempt*, &c. (vol. ii. p. 403.) also Dr. Thomson considers this to be the true composition of this salt. Wishing, however, to determine its nature, as well as that of the salt remaining in the solution from which it is precipitated, I put 200 grains of bipersulphate of mercury into about a quart of cold water; the yellow sulphate precipitated weighed 141·1 grains; the solution was then heated, by which 8·4 grains more were obtained; afterwards sulphuretted hydrogen threw down 14·5 of bisulphuret of mercury.

To ascertain the composition of the yellow sulphate, I heated 100 grains in a solution of soda; the peroxide of mercury separated weighed 86·9 grains; to the solution, after supersaturation with muriatic acid, muriate of barytes was added, and

37·3

37·3 grains of sulphate were precipitated, equivalent to 12·6 of sulphuric acid One hundred grains, therefore, yielded of

Sulphuric acid	12·6
Peroxide of mercury	86·9
Loss	·5
	100·0

I ascertained the quantity of peroxide of mercury also, by decomposing the salt with sulphuretted hydrogen; 100 grains gave 94·8 of bisulphuret of mercury = 88·2 of peroxide. Taking the mean of these experiments, the salt consists of

Sulphuric acid	12·6
Peroxide of mercury	87·5
	100·1

I therefore consider the yellow sulphate of mercury, as a subpersulphate constituted of

Three atoms of sulphuric acid (40×3)	= 120 or	12·2
Four atoms of peroxide of mercury (216×4)	= 864	87·8
	984	100·0

or it may be regarded as consisting of

Two atoms of persulphate of mercury	$(80+432)$	= 512
One atom of dipersulphate	$(40+432)$	= 472
		984

This however is so unusual an atomic constitution, that I have not admitted its existence until after repeated analyses; it will be observed, that if we add the oxygen to the mercury, in Fourcroy's analysis, the resulting peroxide will amount to 87, with which the results of my experiments very nearly agree.

With respect to the sulphuric acid and the peroxide of mercury remaining in solution, and which have been supposed to constitute a peculiar supersalt; it may be observed, that when four atoms of bipersulphate of mercury are acted upon by water, a compound of three atoms of acid and four atoms of oxide is precipitated, while five atoms of sulphuric acid remain in solution: this acid, however, prevents the decomposition of the whole of the bipersulphate by dissolving a portion of it; the quantity remaining in solution depends, to a certain extent, upon that of the water employed; thus, when using a quart of water, as in the above related experiment, nearly 150 of the yellow subpersulphate were precipitated from 200 of the bipersulphate, but when only half the quantity of water was used, 155 were obtained from an equal weight:

weight: in the former experiment, therefore, about one-tenth of the bipersulphate, and in the latter rather less, remained undecomposed.

Having some reason to suppose that the compounds of carbonic acid and mercury had not been sufficiently examined, I collected all the evidence on the subject, which I have been able to obtain from the numerous authors whom I have consulted. Dr. Thomson (*System*, vol. ii. p. 658.) says; "Carbonic acid does not attack mercury, but it may be combined with its oxide by pouring an alkaline carbonate into nitrate of mercury. The precipitate in that case is a white powder, composed according to Bergman of

Mercury....................	90·9
Oxygen and acid.........	9·1
	100·0

"Supposing the carbonate a compound of 1 atom carbonic acid + 1 atom peroxide of mercury, it will consist of

Carbonic acid....................	9·24
Peroxide..........................	90·76
	100·00."

It must however be very evident that the salt obtained by Bergman was not a percarbonate so constituted; and probably it was not a percarbonate at all; for 90·9 of mercury require nearly 7·3 of oxygen for conversion into peroxide, and consequently there is left only 1·8 for carbonic acid: if we suppose it a protocarbonate, it must consist of about 90·5 protoxide and 9·5 carbonic acid; but for reasons, which I shall presently state, I am inclined to believe that it contained no carbonic acid whatever.

In his *Attempt* &c. (vol. ii. p. 397.) Dr. Thomson does not mention any percarbonate of mercury; but he informs us that he obtained a white protocarbonate of mercury, by adding carbonate of soda to a solution of nitrate of mercury; the precipitate lost 14·44 per cent. by solution in nitric acid, and Dr. Thomson considers it, therefore, as a sesquiprotocarbonate of mercury, composed of

One atom and a half of carbonic acid	33 or	13·7
One atom of protoxide of mercury	208	86·3
	241	100·0

Although, as already mentioned, I have referred to many chemical writers, for evidence as to the existence and composition of protocarbonate of mercury, yet except what I have quoted from Dr. Thomson, my researches have been attended with

with but little success; it is indeed true, that Berzelius (*Essai sur la Théorie des Proportions Chimiques*, table, p. 21.) states, that what he calls *carbonas hydrargyrosus*, consists of 9·47 carbonic acid +90·53 protoxide of mercury, or an atom of each; but this, I take it for granted, is merely theoretical composition.

Berthollet (*Mémoires d'Arcueil*, tom. iii. p. 89.) after mentioning the precipitation of pernitrate of mercury, by carbonate of soda, to which I shall again advert: says, "On a fait la même expérience avec une dissolution nitrique de protoxide de mercure. Le précipité était d'un jaune clair; il a fait, après avoir été bien lavé, une vive effervescence avec l'acide nitrique. Lorsqu'on pousse fort loin les lotions, il prend une couleur noirâtre; et même sa surface se noircit lorsqu'on le laisse sous l'eau;" and he afterwards adds, "le protoxide se combine avec lui, [l'acide carbonique,] et peut former un carbonate, lequel cependant peut être décomposé par la seule action de l'eau qui lui enlève l'acide carbonique, quoique difficilement."

Upon considering these statements, I apprehend that Berthollet took the yellow precipitate for a carbonate, and the black one for protoxide of mercury derived from its decomposition.

To procure protocarbonate of mercury, I mixed a solution of carbonate of potash with one of protonitrate of mercury; the precipitate at first produced was of a yellowish colour, and it remained so until excess of the alkaline carbonate was added; it then became immediately of a dark colour, and eventually as black as the precipitate formed by caustic potash: I have therefore no doubt that the yellowish precipitate, first obtained, was a subprotonitrate, and it dissolved in nitric acid without effervescence; if the solution of the nitrate be added to an excess of that of the carbonate, the precipitate is at once black.

Two hundred grains of the precipitate procured with excess of carbonate of potash, and dried by exposure to the air, were dissolved in a weighed vial of dilute nitric acid; the loss of weight was only 0·5 of a grain, and was evidently one of manipulation merely; this experiment I have repeated with similar results.

Under these circumstances, I am of opinion that a white or yellow protocarbonate of mercury cannot be formed; that when the protocarbonate is precipitated it is of a black colour, but loses its carbonic acid by drying in the air.

Berthollet states that percarbonate of mercury cannot be formed; he says indeed, correctly, that when bipermuriate of mercury is treated with carbonate of potash, it is not obtained; but

but he is wrong in supposing that a percarbonate is not procured by adding the carbonate to a solution of pernitrate: I mixed solutions of these salts and obtained a precipitate, which had an ochre yellow colour; it was dried by exposure to the air, lost 4·4 per cent. by dissolving in dilute nitric acid, and the solution when decomposed by soda gave 96·1 of peroxide; this salt is therefore a dipercarbonate, consisting of

Two atoms of peroxide of mercury (216×2)	=	432	or 95·2
One atom of carbonic acid.....................	=	22	4·8
		454	100·0

XXVIII. *Experiments on Vanadiate of Ammonia, and on some other Compounds of Vanadium.* *By Mr.* John Prideaux, *Member of the Plymouth Institution.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

In the absence of detailed information on vanadium, a summary of experiments on the minute scale, on a portion of vanadiate of ammonia, with which I was favoured in a letter from Professor Berzelius, about a month since, may be not unacceptable to your chemical readers. It was accompanied by the information, that the atomic weight of vanadium is 855·87: that it combines with 1, 2 and 3 atoms of oxygen, with the latter quantity forming vanadic acid; that by driving off the ammonia, in an open vessel, the acid would be obtained; and that by the same process, out of access of air, the oxide would be produced.

The acid was the first subject of experiment. It is a dull orange red powder, as described in your last Number, p. 152; and the neutral salts it forms with alkaline and earthy bases are white; which would hardly have been expected, considering the analogies of vanadium with chrome. With excess of acid, however, they are yellow.

1. Vanadiate of potash is very soluble, and showed no disposition to crystallize, although evaporated slowly to dryness, with frequent intervals of cooling, and afterwards redissolved and left to spontaneous evaporation.

2. Vanadiate of soda is scarcely so soluble, but not more disposed to crystallize.

3. The quantities employed amounting to only about 2 grains of each salt, and the indisposition to crystallize being perhaps due to the smallness of the quantity, the two solutions were mixed and abandoned to spontaneous evaporation; the glass

 being

being occasionally made to vibrate, by striking on the edge with a steel blade. The salts were, however, left on the glass in two successive coatings; and a film, which had formed upon the liquor, was studded beneath with brilliant globules, in which no facets could be discovered by the microscope.

4. Vanadiate of ammonia is much less soluble, falling like cream of tartar; it crystallized freely, on cooling, in acute rhombic blades, like spear-heads.

5. Vanadiate of lime is still less soluble than the last, and also crystallized in cooling. Crystalline form not unlike the other, but thicker. The crystals were, however, so irregular that but few could be defined.

6. Vanadiate of barytes. A solution of 1·482 grains of vanadiate of ammonia, containing an atom of water, was mixed with a solution of 1·312 grain of chloride of barium; the whole fluid became full of coagula, which settled, by boiling, into a heavy white power. The residual liquor was evaporated and tested in the usual manner, and the decomposition was found complete, so far as could be ascertained by this mode.

This precipitate affords a convenient method of recovering the vanadic acid used in experiments. Mixing together the various soluble vanadiates, solution of muriate of barytes may be added, in slight excess, keeping the liquor hot. The vanadiate of barytes will quickly subside, and may be readily washed. It is then (whilst quite recent) to be thrown into 40 or 50 parts of water, with a little excess of sulphuric acid; when it will assume the deep orange colour of bi-chromate of potash. It may digest for half an hour, with occasional shaking; when carbonate of ammonia is to be added, a small lump at a time, letting it dissolve gradually, and mixing up the liquor well before each addition, till the liquor is neutral, avoiding much excess of the carbonate. The precipitate will then be white or will become so by boiling in the liquid; and after washing to remove the adhering solution will not be discoloured by fresh sulphuric acid. The liquor will contain sulphate and vanadiate of ammonia, which are to be separated by crystallization.

In the following experiments, solution of vanadiate of ammonia was dropped into excess of the metallic solutions; the persulphate of iron was prepared by heating protosulphate in a test-tube with nitric acid, and contained that acid in excess; —the others were all pure.

	Immediate Effect.	*After* 12 *Hours.*
Sulpate of Zinc.	No precipitate; the liquid assumed a greenish-yellow hue, becoming gradually yellower and turbid.	A pulverulent yellowish-white precipitate; the liquid orange yellow.

Green

	Immediate Effect.	*After 12 Hours.*
Green Sulphate of Iron.	An abundant flocculent dark slate coloured precipitate, leaving the liquid a paler shade of a similar colour, with a tint of green.	The precipitate unchanged; the liquid greener, like solution of sulphate of iron.
Per-Sulphate of Iron.	Little or no precipitate; the liquid deep orange.	Unchanged.
Sulphate of Copper.	An orange-coloured precipitate at first contact, appearing greener on mixing with the blue solution; settled, pale yellowish green, leaving the liquid a deeper shade of the same colour.	The precipitate inclines to orange colour; the liquor remains yellowish green.
Nitrate of Cobalt.	Slight precipitate, pale salmon-coloured, liquid scarcely altered.	Unchanged.
Acetate of Lead.	Precipitate copious and heavy, of nearly the same colour as the above, settling paler and more orange; the liquid colourless.	The precipitate almost bleached, probably by taking down excess of lead.
Corrosive Sublimate.	An orange precipitate; heavy, settling yellower; the liquid dull pale yellowish green.	The precipitate yellow; liquid the colour of chlorine.
Tartar Emetic.	A brownish orange coagulum, redissolved on shaking; and giving that hue to the liquid.	The liquid opalescent, dull pale green by reflected, and orange by transmitted light; no precipitate.

With vanadium as a base, the operations were more embarrassing. The oxides formed simultaneously, during the decomposition of vanadiate of ammonia, and were difficult to separate. One of them would seem to possess acid properties; and the other to be partly converted into that in washing, and partly to combine with it, forming a soluble compound, not precipitated either by alkalies or acids *. The experiments were inconclusive, and the description of them must be very general.

Two portions of vanadiate of ammonia were decomposed by heating in a close platinum crucible: one of them had been recovered from former experiments, by precipitation with barytes, as above described, and probably was not free from sulphuric acid. The other had only been converted into vanadic acid, redissolved in liquid ammonia and crystallized.

The first left an oxide of a deep blue or green colour, almost black. As it was supposed to contain sulphuric acid, it was subjected to that acid, a drop of which turned the part with which it lay in contact, light green; but the addition of a few drops of water, after some hours, to give fluidity to the whole, turned it deep blue, and syrupy, like undiluted sulphate of indigo. After twelve hours digestion, about 30 parts

* See our last Number, p. 152.—Edit.

 more

more water were added, by which it was dissolved; and after the subsidence of a yellow sediment the solution was pale blue and strongly acidulous. The yellow sediment dissolved in liquid ammonia, and was vanadic acid.

Carbonate of barytes in fine powder was added to the solution, to get rid of the excess of sulphuric acid. It produced little effect, till heat was applied; when effervescence took place, the liquid became pistachio-green, and a flocculent dark olive precipitate appeared; which gradually subsided, leaving the liquor still blue and acidulous. Liquid ammonia did not render it neutral, until a nearly black precipitate had fallen in considerable quantity; almost the whole of which was taken up again by ammonia in excess.

In the second attempt the precaution was taken to add carbonate of ammonia in decomposing the salt, that the first impression of heat might fill the crucible with an atmosphere of that substance. The remaining oxide, however, still contained vanadic acid.

The solution in muriatic acid was blue, and could not be neutralized without precipitation. Before neutralization took place, liquid ammonia threw down an olive precipitate, and made the liquor green, as above. The green liquid, poured off, and gradually neutralized with ammonia, gave a dark-brown precipitate. The olive precipitate dissolved green, the brown one blue, in muriatic acid.

After various experiments, in none of which did the oxide of vanadium neutralize the acid employed, or yield with it a crystallized salt, the solutions were mixed together, and thrown into an excess of caustic potass, largely diluted. A deep brown light precipitate fell, doubtless hydrate of the oxide (most probably of the protoxide), leaving the liquor of the same colour but transparent. A portion of this liquor was withdrawn, and mixed with bi-carbonate of potash, when it bleached, but without precipitating. The remainder was then poured off and tried with muriatic acid, in slight excess, which produced the green colour mentioned before, but without disturbing the transparency. Caustic potash was then slowly added, and whilst still acidulous, an inky precipitate fell; after which a reddish brown one followed, when the liquor became neutral.

The reddish-brown and purple precipitates were both soluble in acids, alkalies, and distilled water, and were therefore difficult to wash; both also reddened litmus paper. The brown precipitate became purple on the surface, when no great depth of water lay on it, and at the same time square plates formed in it, very small and thin, iridescent by reflected, but deep yellow

yellow by transmitted light, which dissolved green in muriatic acid without neutralizing it. None of these on being dissolved in alkali and neutralized, produced precipitates in the metallic solutions, similar to those with vanadiate of ammonia, and therefore none appeared to contain vanadic acid; nor did I succeed in obtaining a solution of the brown precipitate in an acid, either neutral or which could be neutralized with ammonia, without precipitation; a difficulty which I attribute to its absorption of oxygen in washing. Attempts to obtain more of it by boiling the other oxides with de-oxydating substances were also frustrated by the difficulty of separating it afterwards.

The solutions and precipitates were then mixed with nitric acid in excess, and boiled together to dryness; redissolved in solution of potash and boiled until colourless; slightly acidulated with muriatic acid; precipitated with muriate of barytes; and reconverted into vanadiate of ammonia, as above. A small bulb was then blown on a bit of glass tube, which was drawn out at half an inch from the bulb, and cut off, in the contracted part, so as just to allow the vanadiate of ammonia to enter. This salt was mixed with about half as much carbonate of ammonia, and put into the bulb, which it filled. The orifice was then nearly closed, at a spirit lamp; the neck thrust into the end of a quill, which was stuffed up with tow from the other end, so as very much to impede the access of air. The bulb was then heated over a spirit lamp, till ammonia ceased to pass off: yet the oxide thus prepared did not yield either a neutral solution, or one disposed to crystallize, with sulphuric acid; and contained itself a large proportion of acid. Nor did it, after various experiments, give results which could elucidate those above related.

Vanadium gives, particularly in the reducing flame, a green bead with borax, of a tint rather yellower than that from chrome.

Upon the whole the brown powder appears to me to be hydrate of the protoxide; the purple, a combination of that substance with the deutoxide; and the crystals, the same combination, but with the protoxide in larger proportion. The deutoxide seems to possess acid properties; to be soluble in acids as well as alkalies; and to yield, with metallic solutions, precipitates different from those with vanadic acid. The oxide, both brown and mixed, dissolves blue in acids; but the solution becomes green by the addition of alkali, though still highly acidulous. The square crystals dissolve green in acids, and the water becomes green when lying upon the surface of the oxide, as it changes to purple. Hence the solution of protoxide,

protoxide, in peroxide of vanadium, would appear to be green when the latter is in excess. But I cannot claim much faith for the inferences from these minute experiments, which must be resumed (unless we have more precise information in the meanwhile) if, by finding vanadium in our own neighbourhood, I can obtain a larger supply. I have examined a considerable number of phosphates and arseniates of lead, in which Mr. Johnstone's description led me to suspect that substance; and some primitive iron ores, yielding a particularly soft metal; but have not hitherto found any traces of vanadium. Yours, &c.

Plymouth, Aug. 10, 1831. J. PRIDEAUX.

XXIX. *Notices respecting New Books.*

The Life of Sir HUMPHRY DAVY, *Bart. LL.D. late President of the Royal Society, Foreign Associate of the Royal Institute of France, &c. &c. By* John Ayrton Paris, *M.D. Cantab. F.R.S. &c. Fellow of the Royal College of Physicians.*

"THE great end of biography," Dr. Paris remarks, "is not to be found, as some would seem to imagine, in a series of dates, or in a collection of gossiping anecdotes and table talk, which, instead of lighting up and vivifying the features, hang as a cloud of dust upon the portrait; but it is to be found in an analysis of human genius, and the development of those elements, to whose varied combinations, and nicely adjusted proportions, the mental habits, and intellectual peculiarities of distinguished men may be readily referred."

Although we admit this to be a just description of the proper aim and end of biography, yet we confess we should have great pleasure in quoting from Dr. Paris a variety of anecdotes which illustrate what he terms "superficial peculiarities;" for they are not only amusing, but they tend, to a certain extent, to elucidate the progress of Sir H. Davy's genius in cultivating the science, whose limits have been so greatly extended by the variety, spendour and importance of his discoveries. In the present instance, however, so much is due to the merits of the philosopher, that we can dwell but little upon the characteristics of the man.

Humphry Davy was born at Penzance in Cornwall, on the 17th of December 1778; his ancestors it appears had long possessed a small estate at Varfell, in the parish of Ludgvan, in the Mount's Bay, on which they resided. His father was a carver of wood; and of him Dr. Paris observes, what indeed may be said of the father of many an illustrious son, that he "is not able to discover that he was remarkable for any peculiarity of intellect; he passed through life without bustle, and quitted it with the usual regrets of friends and relatives." His mother, whose maiden name was Grace Millet, appears

appears to have possessed remarkable placidity of temper, and an amiable and benevolent disposition.

When very young he was placed at the Grammar School in Penzance, whence he went to Truro, where he finished his education. Although quick and industrious in his school exercises, he was found very deficient in the qualifications for the class of his age; but by industry and attention he subsequently acquitted himself to the entire satisfaction of his master.

In his boyish days he was fond of romance, of writing verses and ballads, of fireworks, shooting and fishing. The taste for poetry increased with his years, and Dr. Paris has given some specimens of his poetic talent which are not unworthy of his genius. At twelve years of age he composed an epic poem, of which, however, not even a fragment has been preserved. His love of fishing he always retained: it appears, indeed, to have increased, rather than diminished with his years.—"His temper during youth," says Dr. Paris, "is represented as mild and amiable. He never suppressed his feelings, but every action was marked by ingenuousness and candour."

In February 1795 he was apprenticed to Mr. Borlase, a surgeon and apothecary, and afterwards a physician at Penzance; and although his mind had been for some time engrossed with philosophical pursuits, it does not appear that he had any decided turn for Chemistry, until after he had been placed with Mr. Borlase; but he then commenced the study of it with ardour. "As far as can be ascertained," says Dr. Paris, "one of the first original experiments on chemistry performed by him at Penzance, was for the purpose of discovering the quality of the air contained in the bladders of sea-weed, in order to obtain results in support of a favourite theory of light; and to ascertain whether, as land vegetables are the renovators of the atmosphere of land animals, sea vegetables might not be the preservers of the equilibrium of the atmosphere of the ocean. From these experiments he concluded, that the different orders of the marine Cryptogamia were capable of decomposing water, when assisted by the attraction of light for oxygen." He seems also to have paid attention to Geology while with Mr. Borlase;—during his walks his usual companion was a hammer, with which he procured specimens from the rocks on the beach.—"In short," observes Dr. Paris, "it would appear that, at this period, he paid much more attention to philosophy than to physic; that he thought more of the bowels of the earth than of the stomachs of his patients; and that when he should be bleeding the sick, he was opening veins in the granite."

During his stay with Mr. Borlase he became acquainted with Mr. Davies Gilbert (then Giddy), who accidentally hearing that he was fond of chemical experiments, expressed a desire to have some conversation with him; and during this he soon discovered ample evidence of young Davy's singular genius, and the consequence was an offer of the use of his library or any other assistance he might require for the pursuit of his studies.

In October 1798 Davy quitted Penzance to superintend the Pneu-

Pneumatic Institution at Bristol, established by Dr. Beddoes for the purpose of investigating the medicinal powers of factitious airs or gases. "It is now generally acknowledged," says Dr. Paris, "that the Art of Physic has not derived any direct advantage from the application of a class of agents, which undoubtedly held forth the fairest promise of benefit." "The investigation, however," he continues, "into the nature and composition of the gases paved the way to some new and important discoveries in science; so that to borrow a Baconian metaphor, although our philosophers failed in obtaining the treasure for which they so eagerly dug, they at least, by turning up and pulverizing the soil, rendered it fertile. The ingenuity of the chemist will for ever remain on record; the phantoms of physicians have vanished into air."

In a letter to Mr. Davies Gilbert, dated Clifton, November 12, 1798, Davy says, "We are printing in Bristol the first volume of the West Country Collections, which will I suppose be out in the beginning of January." Dr. Paris informs us that "the work announced in the above letter was published in the commencement of the year 1799, under the title of 'Contributions to Physical and Medical Knowledge, principally from the West of England; collected by Thomas Beddoes, M.D.'"

The following are Dr. Paris's observations on this work: "The first two hundred pages, constituting very nearly half the volume, are the composition of Davy, and consist of essays 'On Heat, Light, and the Combination of Light;' 'On Phos-oxygen, or Oxygen and its Combinations;' and 'On the Theory of Respiration.'

"His first essay commences with an experiment, in order to show that light is not, as Lavoisier supposed, a modification or an effect, of heat, but matter of a peculiar kind, *sui generis*, which, when moving through space, or in a state of projection, is capable of becoming the source of a numerous class of our sensations."

"A small gunlock was armed with an excellent flint, and on being snapped in an exhausted receiver, did not produce any light. The experiment was repeated in carbonic acid, and with a similar result. Small particles were in each case separated from the steel, which, on microscopic examination, evidently appeared to have undergone fusion. Whence Davy argued, that light cannot be caloric in a state of projection, or it must have been produced in these experiments, where heat existed to an extent sufficient to fuse steel. Nor, that it can be, as some have supposed, a vibration of the imaginary fluid æther; for, granting the existence of such a fluid, it must have been present in the receiver. If, then, light be neither caloric in a state of projection, nor the vibration of an imaginary æther, it must, he says, be a substance *sui generis*."

"With regard to caloric, his opinion that it is not, like light, material, has been already noticed. In the present essay he maintains the proposition by the same method of reasoning as that by which he attempts to establish the materiality of light, and which mathematicians have termed the 'reductio ad absurdum.'"

"In his chapter on Light and its Combinations," he indulges in specu-

speculations of the wildest nature, although it must be confessed that he has infused an interest into them which might almost be called dramatic. They are certainly highly characteristic of that enlightened fancy, which was perpetually on the wing, and whose flight, when afterwards tempered and directed by judgement, enabled him to abstract the richest treasures from the recesses of abstract truth."

"Taking it for granted that caloric has no existence as a material body, or, in other words that the phænomena of repulsion do not depend upon the agency of a peculiar fluid, and that on the contrary, light is a subtile fluid acting on our organs of vision *only when in a state of repulsive projection;* he proceeds to examine the French theory of combustion; the defects of which he considers to arise from the assumption of the imaginary fluid *caloric*, and the total neglect of *light*. He conceives that the light evolved during combustion previously existed in the oxygen gas, which he therefore proposes for the future to call PHOS-OXYGEN."

"In following up this question, he would seem to consider light as the *Anima Mundi*, diffusing through the universe not only organization, but even animation and perception."

"*Phos-oxygen*, he considers as capable of combining with additional proportions of light, and of thus becoming '*luminated phos-oxygen!*' From the decomposition of which, and the consequent liberation of light, he seeks to explain many of the most recondite phænomena of nature."

"We cannot but admire the eagerness with which he enlists known facts into his service, and the boldness with which he ranges the wilds of creation in search of analogies for the support and illustration of his views. He imagines that the *phos-oxygen* when thus *luminated*, must necessarily have its specific gravity considerably diminished by the combination, and that it will therefore occupy the higher regions of the atmosphere; hence, he says, it is that combustion takes place at the tops of mountains at a lower temperature than in the plains, and with a greater liberation of light. The hydrogen which is disengaged from the surface of the earth, he supposes will rise until it comes into contact with this *luminated phos-oxygen*, when by its attracting the oxygen to form water, the light will be set free, and give origin to the phænomena of fiery meteors at a great altitude."

"The phænomena termed 'Phosphorescence,' or that luminous appearance which certain bodies exhibit after exposure to heat, is attributed by this theory to the light, which may be supposed to quit such substances as soon as its particles have acquired repulsive motion by elevation of temperature."—"The electric fluid is considered as light in a condensed state, or, in other words, in that peculiar state in which it is not supplied with a repulsive motion sufficiently energetic to impart projection to its particles; for he observes, that its chemical action upon bodies is similar to that of light; and when supplied with repulsive motion by friction, or by the contact of bodies from which it is capable of subtracting it, it

loses the projectile form, and becomes perceptible as light. It is extremely probable, he adds, that the great quantity of this fluid almost every where diffused over our earth is produced by the condensation of light, in consequence of the subtraction of its repulsive motion by black and dark bodies; while it may again recover the projectile force by the repulsive motion of the poles, caused by the revolution of the earth on its axis, and thus appear again in the state of sensible light; and hence the phænomenon of the *Aurora Borealis*, or Northern Lights."

"In considering the theory of respiration, he supposes that *phos-oxygen* combines with the venous blood without decomposition, but that on reaching the brain, the light is liberated in the form of electricity, which he believes to be identical with the nervous fluid. On this supposition, sensations and ideas are nothing more than motions of the nervous æther; or light exciting the medullary substance of the nerves and brain into sensitive action!"

"He thinks it would be worth while to try, by a very sensible electrometer, whether an insulated muscle, when stimulated into action, would not give indications of the liberation of electric fluid, although he suspects that in man the quantity is probably too small, and two slowly liberated, to be ascertainable. In the torpedo, and in some other animals, however, it is unquestionably given out perceptibly during animal action."

"When any considerable change takes place in the organic matter of the body, so as to destroy the powers of life, new chemical attractions and repulsions take place, and the different principles of which the body is composed enter into new combinations. In this process, which is called putrefaction, Davy, in pursuance of this theory, thinks that in land animals the latent heat of the system enters into new combinations with oxygen and nitrogen, but that in fish no such combinations occur, and hence the luminous appearance which accompanies their putrefaction."

Dr. Paris very justly characterizes these essays as extraordinary; but "I am not quite sure," he adds, "that amidst all the meteors of his fancy there may not be a gleam of truth. I allude to his theory of Respiration: it certainly does not square with the physiological opinions of the day; nor did that of Newton, when he conjectured that water might contain an inflammable element; but it was the refraction of a great truth, at that time below the horizon." We admit with Dr. Paris, that the theory of *phos-oxygen* and *luminated phos-oxygen* has scarcely a parallel in extravagance and absurdity, and with him we also "happen to know that in after life Davy bitterly regretted that he had so committed himself; any allusion to the subject became a source of painful irritation."— This was precisely the effect produced upon him by the notice which Chenevix took of his theory in his treatise on nomenclature. After all, Dr. Paris rightly observes, "the reader, however, will be disposed to treat him with all tenderness when he remembers that the author of these essays was barely eighteen years of age." In a letter to Mr. Gilbert, dated Clifton, February 22, 1799, he gives

gives an account of the discovery that two pieces of canes rubbed together gave a faint light, which he shows was occasioned by the silica contained in the epidermis. In the same letter he announces a more important fact—"I made a discovery yesterday, which proves how necessary it is to repeat experiments. The gaseous oxide of azote is perfectly respirable when pure. It is never deleterious but when it contains nitrous gas. I have found a mode of obtaining it pure, and I breathed to-day, in the presence of Dr. Beddoes and some others, sixteen quarts of it for near seven minutes. It appears to support life longer than even oxygen gas, and absolutely intoxicated me. Pure oxygen gas produced no alteration in my pulse, nor any other material effect; whereas this gas raised my pulse upwards of twenty strokes, made me dance about the laboratory like a madman, and has kept my spirits in a glow ever since. Is not this a proof of the truth of my theory of respiration? for this gas contains more light in proportion to its oxygen than any other, and I hope it will prove a most valuable medicine."

In the year 1800, appeared in one octavo volume, 'Researches, Chemical and Philosophical; chiefly concerning Nitrous Oxide, or Dephlogisticated Nitrous Air and its Respiration. By Humphry Davy, Superintendant of the Medical Pneumatic Institution.'—This is a work containing the results of great labour and numerous experiments. Dr. Paris remarks that "it may perhaps appear extraordinary to the reader of the 'Researches,' that although they were published not more than eighteen months after the appearance of his 'Essays on Heat and Light,' no allusion is made in them either to his theory, or his new nomenclature. In relating his experiments upon respiration, he employs the conventional language of the schools, and the word 'phos-oxygen' does not once occur in the volume. This is fully explained in a communication made by him to Mr. Nicholson, and which was printed in his Journal a short time after the publication of his Essays in the West Country Contributions; in which he says, 'As facts have occurred to me with regard to the decomposition of bodies, which I had supposed to contain light, without any luminous appearance, I beg to be considered as a *sceptic* with respect to my own particular theory of the combinations of light, until I shall have satisfactorily explained these anomalies by fresh experiments. On account of this scepticism, and for other reasons, I shall in future use the common nomenclature; excepting that, as my discoveries concerning the gaseous oxide would render it highly improper to call a principle, which in one of its combinations is capable of being absorbed by venous blood, and of increasing the powers of life, *azote*,—I shall name it, with Dr. Pearson, Chaptal and others, NITROGENE; and the *gaseous oxide of azote* I shall call NITROUS OXIDE."

"There is one circumstance connected with the views entertained in this work," observes Dr. Paris, "which must not be passed over without notice. In several passages he advocates the theory of the atmosphere being a *chemical compound* of oxygen and nitrogen;

gen; whereas in later years he was amongst the first to insist upon its being simply a mechanical mixture of these gases." Soon after the appearance of the 'Researches,' Davy was invited by Count Rumford to the Royal Institution, which had been recently formed under his auspices; he arrived at the Institution on the 11th of March 1801, in the capacities of Assistant Lecturer on Chemistry, Director of the Laboratory, and Assistant Editor of the Journal of the Institution: in about six weeks he was appointed Lecturer on Chemistry instead of Assistant; and in May 1802 he was styled Professor of Chemistry.

On the 21st of January 1801, Davy gave the introductory lecture to the first regular course; this was exceedingly well received by a numerous audience, and was printed; he had previously given occasional lectures, but this must be considered as the commencement of his splendid career. This course of lectures, as appears from a printed syllabus, was divided into three parts:—the chemistry of ponderable substances; the chemistry of imponderable substances; and the chemistry of the arts. From this period he continued regularly to increase in fame and popularity; his first paper in the Journal of the Royal Institution is entitled 'An Account of a New Eudiometer;' this was simply a small graduated tube divided into 100 parts, immersed into a solution of protosulphate or protomuriate of iron, impregnated with nitric oxide: as Dr. Priestley had not only employed this gas as a eudiometrical substance, but had shown the power of sulphate of iron in absorbing it, Dr. Paris very justly remarks, that this test "can only be regarded as a convenient modification of that of Priestley, in which nitrous gas was presented to the atmospheric air to be examined, without the intervention of any third body."

The Royal Institution Journal contains several other communications from him, under the titles of 'Observations on different methods of obtaining Gallic Acid;' 'On the Processes of Tanning, &c.' All the new facts were embodied in an elaborate memoir, and read before the Royal Society, of which he was elected a Fellow on the 17th of November, 1803; in 1801 he had communicated to the Society his first paper, entitled 'An Account of some Galvanic Combinations, formed by single metallic plates and fluids, analogous to the Galvanic Apparatus of M. Volta.' After this followed the paper to which we have above alluded, and then 'An Account of some Analytical Experiments on a Mineral Production from Devonshire, consisting principally of Alumina and Water.' The Rev. William Gregor had detected the presence of fluoric acid in this substance. "The subsequent experiments of Berzelius, however," Dr. Paris remarks, "cleared away the obscurity in which the subject was still involved. He showed that this mineral not only contained in its composition a small portion of the *neutral fluate of alumina*, but he demonstrated the presence of a subphosphate of that earth, to no inconsiderable amount. Much has been said of the error committed on this occasion by Davy, in overlooking thirty-three per cent. of phosphoric acid; but the *phosphate of alumina* is a body that might very

very easily have escaped notice at a period when mineral analysis was in a far less advanced state than it is at present." We profess utter ignorance as to the parties who have said much on this occasion: the mistake might be readily pardoned at all times by those acquainted with the difficulties of chemical analysis; this will be still more readily admitted, when it is recollected that Berzelius himself, certainly one of the most skilful analysts that ever existed, actually overlooked sixteen per cent. of the same acid in uranite.

His next paper was entitled 'An Account of analyzing Stones containing a fixed Alkali, by means of Boracic Acid': for this and the above-mentioned papers the Society awarded him the Copley medal. This communication was followed by the Bakerian lecture, read on the 20th of November 1806. According to Dr. Paris, this paper "unfolded the mysteries of general voltaic action; and as far as theory goes, may be almost said to have perfected our knowledge of the chemical agencies of the pile. This grand display of scientific light burst over Europe like a spendid meteor, throwing its radiance into the deepest recesses, and opening to the view of the philosopher new and splendid regions." The subjects investigated in this memoir are arranged by Dr. Paris as follows:—the changes produced in water by electricity; the agencies of electricity in the decomposition of various compound bodies; the transfer of certain constituent parts of bodies by the action of electricity; the passage of acids, alkalies and other substances, through various attracting chemical menstrua, by means of electricity; general observations on these phænomena, and on the mode of decomposition and transition; the general principles of the chemical changes produced by electricity; the relations between the electrical energies of bodies and their chemical affinities; the mode of the action of the pile of Volta, with experimental elucidations; general illustrations and applications of the foregoing facts and principles. Dr. Paris gives a very able analysis of this paper, which we should be glad to copy if our space would allow. It is no small commendation of this paper that the author received for it the prize of the French Institute.

Davy's second Bakerian Lecture was read on the 19th of November 1807. Of this lecture Dr. Paris also gives an analysis, for which we must refer to his book: to stamp the value and importance of this communication, it would be sufficient to say that it announces the discovery of the metallic bases of the alkalies, potash and soda. On this subject Dr. Paris well remarks, "thus then was a discovery effected, and at once rendered complete, which all the chemists in Europe had vainly attempted to accomplish. The alkalies had been tortured by every variety of experiment which ingenuity could suggest, or perseverance perform, but all to no purpose; nor was the pursuit abandoned until indefatigable effort had wrecked the patience and exhausted the every resource of the experimentalist. Such was the disheartening, and almost forlorn condition of the philosopher, when Davy entered the field:—he created new instruments, new powers, and fresh resources; and Nature, thus interrogated on a different plan, at once revealed her long cherished secret."

It is observed by Davy in his Bakerian Lecture, that "an account of the

the manipulations employed and the difficulties overcome would exceed the limits of a lecture." Well knowing how valuable every minute circumstance is to the chemist, Dr. Paris searched into the archives of the Institution; the result of the examination of the Laboratory Register, we shall give in Dr. Paris's words: "It appears from this register that Davy commenced his inquiries into the composition of potash on the 16th, and obtained his great result on the 19th of October, 1807 *. His first experiments, however, evidently did not suggest the truth: he does not appear to have suspected the nature of the alkaline base until his last experiment, when the truth flashed upon him in the full blaze of discovery. His first note, dated the 16th, leads us to infer that he acted on a solid piece of potash, under the surface of alcohol, and several other liquids in which the alkali was not soluble; and that he obtained gaseous matter, which he called at the moment '*alkaligen gas*,' and which he appears to have examined most closely, without arriving at any conclusion as to its nature. On the following day, he for the first time would seem to have developed potassium by electric action on potash under oil of turpentine, for the note records the fact of '*the* globules giving out gas by water, which gas *burnt* in contact with air;' and then follows a query, 'Does *it* (the matter of the globules) not form gaseous compounds with æther, alcohol, and the oils?' Here, then, he evidently imagined, that the matter of the globules, which he had never obtained from potash, except when acted upon under oil of turpentine, had formed gaseous compounds with the æther, alcohol and oils, in his previous experiments, and given origin to that which he had termed '*alkaligen gas*.'"

"He then leaves the consideration of this gas, and attacks the unknown globules, which probably did not present any metallic appearance under the circumstances in which he first saw them, for they must have been as minute as grains of sand. I rather think that he commenced his examination by introducing a globule of mercury, and uniting it with a globule of the unknown substance; for his note says, 'Action of the substance on mercury, forms with it a solid amalgam, which soon loses its *alkaligen* in the air;' and from the note which succeeds, he evidently considered this *alkaligen* (potassium) as volatile, as he says "it soon flies off on exposure to the air."

"October 19.—It is probable that in consequence of the property which the unknown substance displayed of amalgamating with mercury, he devised his experiment of the 19th. He took a small glass tube, about the size and shape of a thimble, into which he fused a platinum wire, and passed it through the closed end. He then put a piece of pure potash into this tube, and fused it into a mass about the wire, so as entirely to defend it from the mercury afterwards to be used. When cold, the potash was solid, but containing moisture enough to give it a conducting power; he then filled the rest of the tube with mercury, and inverted it over the trough; the apparatus

* On the same day he decomposed soda, with somewhat different phænomena.

ratus being thus arranged, he made the wire and the mercury alternately positive and negative." Dr. Paris then gives an engraving of the autograph account of the results, the substance of which is as follows:—"When potash was introduced into a tube having a platina wire attached to it —so— and fused into the tube so as to be a conductor, *i. e.* so as to contain just water enough, though solid, and inserted over mercury, when the platina was made negative, no gas was formed, and the mercury became oxydated, and a small quantity of the *alkaligen* was produced round the platina wire, as was evident from its quick inflammation by the action of water. When the mercury was made the negative, gas was developed in great quantities from the positive wire, and none from the negative mercury, and this proved to be pure oxygene.—A CAPITAL EXPERIMENT, PROVING THE DECOMPOSITION OF POTASH."

On the subject of this great discovery, Dr. Paris observes, "In the progress of our ascent, it is refreshing to pause occasionally, and to cast a glance at the horizon, which widens at every increase of our elevation. By the decomposition of the alkalies and earths, what an immense stride has been made in the investigation of nature! In sciences kindred to chemistry, the knowledge of the composition of these bodies, and the analogies arising from it, have opened new views and led to the solution of many problems. In geology, for instance, has it not shown, that agents may have operated in the formation of rocks and earths, which had not been previously known to exist? It is evident that the metals of the earths cannot remain at the surface of our globe; but it is probable that they may constitute a part of its interior; and such an assumption would at once offer a plausible theory in explanation of the phænomena of volcanoes, the formation of lavas, and the excitement and effects of subterranean heat, and might even lead to a general theory in geology."

[To be continued.]

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 2nd.—A PAPER was read, "On the Caves and Fissures in the Western District of the Mendip Hills." By the Rev. David Williams, A.M. F.G.S., Rector of the parishes of Bleadon and Kingston-Seamoor, in the County of Somerset. Communicated by Davies Gilbert, Esq. V.P.R.S.

The first cavern described in this paper is situated at Uphill, at the very western extremity of the Mendip Hills. Its present entrance is about midway in a mural face of transition limestone, about a hundred feet high. The fissure leading into it is nearly vertical, and was discovered by some quarry-men casually intersecting it. Some bones and teeth being found there, the author was induced to pursue the exploration of the fissure; in the course of which he discovered bones of the rhinoceros, hyæna, bear, ox, horse, hog, fox, polecat, rat and mouse, and also of birds. The bones of

of the animals of the larger species were so gnawed and splintered, and evidently of such ancient fracture, that no doubt could exist of the cave having been a hyæna's den, similar to Kirkdale and Kent's Hole. All the ancient remains were found in the upper regions of the fissure, and were so firmly imbedded in the detritus, as not to be extracted without difficulty with the pick-axe. Further on he found a wet tenacious loam, abounding with an innumerable quantity of bones, belonging exclusively to birds. After working six days he came to a cavern, ten or twelve feet high, extending about forty feet from north to south, and varying from eight to twenty feet from east to west; the floor of which was covered with bones of sheep: and on digging into the mud and sand of which it consisted, the bones of sheep, birds, cuttle-fish, and foxes, were discovered. Some fine stalactites depended from the roof, and partial spots of stalagmite appeared on the floor. In a fissure that branched from the mouth of the main entrance there were found, among the sand, a piece of black Roman pottery, and two coins, one of Didius Julianus, and the other of Julia Mammæa, together with bones of sheep, cuttle-fish, foxes, and birds.

The author considers that there exist evidences of the operation of water at three distinct periods of time:—the first indicated by the bones of the hyæna, and the other gnawed bones firmly imbedded in the diluvial detritus: the second, when sand was deposited by the sea in the second fissure, that washed in through the vertical chimney, and that inundated the whole valley up to Glastonbury: the third irruption of the sea occurring within these fifteen hundred years, and choking up the adit from the level by which the sheep and foxes had entered, floating in the bones of the cuttle-fish, and depositing the thin crust of mud which covered the sand. The coins and pottery he supposes to have been introduced through this entrance from the level.

The author next gives an account of the Hutton caverns, situated on the northern escarpment of the range, commonly called Bleadon Hill. This cavern had been discovered some time ago and noticed by Mr. Catcott in his "Treatise on the Deluge:" but afterwards it became inaccessible by the falling in of the roof and sides. The author, led by some indications of pieces of ancient bones in the rubbish of some old pits, sought for this cavern by sinking a shaft, and succeeded in opening into it. The chambers he reached are probably the western extremity of a very extensive range of caverns, occurring in a region bearing marks of great disturbance, abounding in chasms and fissures, and containing a great number of bones. The principal of those discovered belong to the elephant, tiger, hyæna, wolf, boar, horse, fox, hare, rabbit, rat, mouse, and bird. No trace of the bones of the ox were discovered here, although in the cave at Banwell Hill, about a mile distant, they abound; while, on the other hand, no vestige of the horse is met with.

Among the remarkable specimens found in the Hutton caverns were the milk-teeth and other remains of a calf elephant about two years

years old, and those of a young tiger just shedding its milk-teeth; and also the molares of a young horse that were casting their coronary surfaces;—the remains of two hyænas of the extinct species; and two or three balls of album græcum.

The Banwell caves, lying about a mile to the east of Hutton, are next described. They are the property of the present Bishop of Bath and Wells; and contain remains of the bear, wolf, fox, deer, and ox. Of the bear there are at least two species; one of which appears to be the *Ursus spelæus* of Blumenbach, and must have been an animal of immense size and strength. These remains were, in general, not associated according to the animals they belonged to, but indiscriminately dispersed: thus the head of a bear lay by the femur of an ox, and the jaw of a wolf lay by the antler of a deer. Hence the author infers that these bones, after accumulating for ages, were carried in by a tumultuous rush of waters, and mingled together before their final deposition. He concludes that the several animals whose remains are deposited in the Banwell and Burlington caves belonged to a very different age and period from those found at Hutton and Uphill.

An account is also given of two caves at Burrington Coomb, lying about six miles to the east of Banwell, in one of which, though similar in appearance to the caves already described, no antediluvian remains of animals have been found. Several human skeletons, and flint knives and celts, were discovered there by Mr. Williams; from which it has been inferred that it had formerly been used as a burying-ground. In the upper caverns, remains of the bear, elk and polecat, were discovered; the two former evidently of the extinct species.

June 9.—A paper was read, entitled "Researches in Physical Astronomy." By J. W. Lubbock, Esq., V.P., and Treasurer of the Royal Society.

The author extends, in the present paper, the equations he has already given for determining the planetary inequalities, as far as the terms depending on the squares and products of the eccentricities, to the terms depending on the cubes of the eccentricities and quantities of that order, which he does by means of a table, similar to the one given in his lunar theory; and applies them particularly to the determination of the great inequality of Jupiter, or at least such part of it as depends on the first power of the disturbing force. That part which depends on the square of the disturbing force may, he thinks, be most easily calculated by the methods given in his lunar theory. He recommends it as particularly convenient to designate the arguments of the planetary disturbances by indices. The bulk of the paper is occupied by the tables, and by examples demonstrating their use.

A paper was read, "On the Theory of the Elliptic Transcendents." By James Ivory, A.M., F.R.S., &c.

Fagnani discovered that the two arcs of the periphery of a given ellipse may be determined in many ways, so that their difference shall be equal to an assignable straight line; and proved that any arc of a lemniscate, like that of a circle, may be multiplied any number of

of times, or may be subdivided into any number of equal parts, by finite algebraic equations. What he had accomplished with respect to the arcs of the lemniscates, which are expressed by a particular elliptic integral, Euler extended to all transcendents of the same class. Landen showed that the arcs of the hyperbola may be reduced, by a proper transformation, to those of an ellipse. Lagrange furnished us with a general method for changing an elliptic function into another having a different modulus; a process which greatly facilitates the numerical calculation of this class of integrals. Legendre distributed the elliptic functions into distinct classes, and reduced them to a regular theory, developing many of their properties which were before unknown, and introducing many important additions and improvements in the theory. Mr. Abel of Christiania happily conceived the idea of expressing the amplitude of an elliptic function in terms of the function itself, which led to the discovery of many new and useful properties. Mr. Jacobi proved, by a different method, that an elliptic function may be transformed in innumerable ways into another similar function, to which it bears constantly the same proportion. But his demonstrations require long and complicated calculations; and the train of deductions he pursues does not lead naturally to the truths which are proved, nor does it present in a connected view all the conclusions which the theory embraces. The author of the present paper gives a comprehensive view of the theory in its full extent, and deduces all the connected truths from the same principle. He finds that the sines or cosines of the amplitudes, used in the transformations, are analogous to the sines or cosines of two circular arcs, one of which is a multiple of the other; so that the former quantities are changed into the latter when the modulus is supposed to vanish in the algebraic expression. Hence he is enabled to transfer to the elliptic transcendents the same methods of investigation that succeed in the circle: a procedure which renders the demonstrations considerably shorter, and which removes most of the difficulties, in consequence of the close analogy that subsists between the two cases.

A paper was read, entitled, "An Experimental Investigation of the Phænomena of Endosmose and Exosmose." By William Ritchie, Esq., M.A., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain.

Mr. Porret had, in the year 1816, announced the discovery, that if a vessel containing water be divided into two compartments by a diaphragm of bladder, and placed in the voltaic circuit, the water would rise on the negative side above its level in the positive compartment. M. Dutrochet discovered, that if alcohol be placed in one of the chambers, and water in the other, without employing the voltaic battery, the water will percolate through the bladder, and the fluid will rise in the chamber containing the alcohol: an action to which he gave the names of *Endosmose* and *Exosmose*, according to its direction with regard to the side of the membrane considered; comparing its two sides to those of a Leyden jar in opposite electrical states. This electrical theory has been combated by M. Poisson: but the true explanation of this singular phænomenon does not appear to have been yet given.

The experiments of the author, of which an account is given in this paper, were made with a glass tube, about an inch in diameter, one end being drawn out into a slender tube of the interior diameter of one eighth of an inch, and having a piece of bladder tied over the other end. When this *Endosmometer*, as it has been called, is by means of a small funnel introduced into the narrow end filled with alcohol, and immersed in water, the water penetrates through the bladder, and the spirit rises rapidly in the narrow stem. The author found on trial that this action was apparently not affected by a powerful current of voltaic electricity passed through the bladder, by introducing positive and negative wires on both sides of it. On substituting a strong solution of sulphate of zinc for the alcohol, the same negative result was obtained.

The author considers the action of the animal membrane to be the consequence of its strong attraction for water, an attraction to which it owes its hygrometric properties: while, on the other hand, the membrane has no attraction for alcohol, which has itself a powerful attraction for water. The water, therefore, finds its way easily through the membrane, and uniting with the alcohol, is carried off by it, and diffused through the liquid, making room for the other portions that successively come over. Whalebone and quills have similar hygrometric properties, and may be substituted for membranes with the same effect. All substances readily soluble in water give rise to the phenomena of endosmose, on the same principle as alcohol, such as gum, sugar, and salts. The phænomenon bears a striking resemblance to the rise of the sap in the capillary vessels of plants, both being probably dependent on the same principle; the filamentous texture of the roots performing the function of the membrane, and the contained sap that of the attractive fluid; by the agency of which the external moisture of the earth is imbibed and raised into the interior of the plant.

June 16.—A paper was read, "On the Tides in the Port of London." By J. W. Lubbock, Esq., V.P., and Treas. R.S.

This paper contains a discussion of observations of the tides made at the London Docks, and registered in various Tables, showing the time and height of high water, not only at different periods of the moon's age, but also for the different months of the year, for every minute of the moon's parallax, and for every three degrees of her declination. The tables themselves were registered by Mr. Dessiou of the Admiralty; but the arrangement of the tables and the methods employed are due to the author. The tides in the river Thames are extremely regular; and as the rise is considerable, the observations on them are easily made. Those at the London Docks present an uninterrupted series from the opening of the Docks in 1804 to the present time; which is more extensive than any extant, with the exception only of that made at Brest by order of the French Government. Some observations are also given of the tides made during one year at the East India Docks, under the superintendence of Captain Eastfield, and which were undertaken at the suggestion of the author, and made with extreme care.

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The author gives an account of the mode by which the several tables were constructed; and enters at length into the various mathematical considerations which the subject involves.

The author was enabled, by the kindness of the Chairman and Directors of the London Dock Company, to present to the Society the books containing the complete series of original observations on the tides referred to in this paper.

A paper was read, "On the Friction of Fluids." By George Rennie, Esq., V.P.R.S.

The object of the author in this paper is to trace the relation subsisting between the different quantities of water discharged by orifices and tubes, and the retardations arising from the friction of the fluid. The results of the experiments hitherto made with a view of ascertaining the effects of the friction attending the mutual motion of solids and fluids, are exceedingly discordant, and therefore undeserving of confidence. Whether, for example, the retardation from friction be proportional to the surfaces, or to the velocities, are points by no means satisfactorily determined.

The experiments of the author were designed to measure the retardations experienced by solids moving in fluids at rest; and of fluids moving over solids. For this purpose, he employed a cylinder of wood, about eleven inches in diameter and two feet in length, traversed by an iron axle, upon the upper part of which a small pulley was fixed. A fine flexible silken cord was wound round the pulley, at one end, and had a weight attached to the other end. A frame was provided, allowing the apparatus to slide up and down; and the cylinder to be immersed at various depths into the river Thames. When the velocities were small, the retardation was found to be nearly as the surface: but with great velocities it appears to have but little relation to the extent of the surface immersed. The resistances of iron discs and wooden globes revolving in water were found to be as the squares of the velocities.

From the experiments made on the quantities of water discharged by orifices of different shapes and sizes from vessels kept constantly full, the author concludes, that they are in the ratio of the areas of the orifices, independently of their shape; and nearly as the square roots of the heights. In pipes bent at various angles the retardation occasioned by the flexure was not in proportion to their number.

A paper was read, "On the Sources and Nature of the Powers on which the Circulation of the Blood depends." By A. P. Wilson Philip, M.D. F.R.S. L. & Ed.

In the first part of this paper the author discusses the opinions which ascribe the powers that maintain the circulation in the veins to the elasticity of the heart, the resilience of the lungs, and the dilatation of the thoracic cavity in the act of inspiration. He shows experimentally that the circulation continues unimpaired when all those causes have ceased to operate; and that the very structure of the veins, the coats of which are so pliable as to collapse by their own weight, when empty, renders it impossible that the motion of the

the blood could be maintained in them by any cause corresponding to a power of suction in the heart.

The latter part of the paper is occupied by an inquiry into the sources and nature of the powers which really support the circulation of the blood. The capillaries, he observes, maintain the motion of their blood long after the heart has ceased to beat; this motion not being immediately affected even by the entire removal of the heart; but being accelerated, retarded, or arrested, according as the action of the capillaries is increased, impaired, or destroyed, by agents of which the operation is wholly confined to the vessels themselves. As the destruction of the heart does not immediately influence the motion of the blood in the capillaries, so the action of this organ, when in full vigour, can produce no motion of the blood in the capillaries, when these vessels are themselves deprived of power. Experiments are related with the view of proving that the arteries and veins, and more particularly the latter, are also capable of carrying on the blood they contain, even in opposition to the force of gravitation, with the greatest ease; and without the aid of any extraneous power. With regard to the nature of the power exerted by the blood-vessels, the author shows that the capillaries are as readily influenced by stimulants and by sedatives, as the heart itself; and that the arteries and veins may also be made to obey the action of stimulants; and further, that the power of the vessels bears the same relation to the nervous system as that of the heart, which is peculiar, and very different from the relation subsisting between that system and the muscles of voluntary motion. From the whole of the facts and experiments stated in this paper, the author deduces the conclusions, that the circulation is maintained by the combined power of the heart and blood-vessels, and that the power of both is a muscular power.

ZOOLOGICAL SOCIETY.

June 14, 1831. Joshua Brookes, Esq. in the Chair.

A letter addressed to the Secretary of the Society by Charles Telfair, Esq., Corr. Memb. Z. S., dated Port Louis, December 15th, 1830, was read. It referred to previous unsuccessful attempts on the part of the Society's valuable correspondent to transport from the Mauritius to England living *Gouramies* and *Tanrecs*, and promised a repetition of the experiment. Mr. Telfair states that he has now a pair of living *Tanrecs* fully grown ready to send to England when he can place them under proper care. "They live on boiled rice, but will probably not exist long upon that alone, as their natural food is chiefly composed of worms, insects, lizards, and the eggs of snails, of which it would be difficult to carry a sufficient supply in a living state on board ship. Fresh supplies might, however, be obtained at Madagascar or the Cape of Good Hope, at St. Helena, Ascension, and the Cape de Verd Islands; and the animals might thus arrive in good health in England, where they would probably survive for some time burrowing under a dungheap, or living in straw in a hot-house or greenhouse.

house. An opportunity would thus be furnished of observing their habits. In the Mauritius they sleep through the greater part of the winter, from April to November, and are only to be found when summer heat is felt, which being generally ushered in by an electric state of the atmosphere, the negroes (with whom they are a favourite food) say they are awakened by the peals of thunder which precede the summer storms or 'pluies d'orage.' Even in summer they are not often seen beyond the holes in which they burrow, except at night. Their favourite haunts are among the old roots of clumps of bamboos. They have a very overpowering smell of musk at all times, which is increased to an extraordinary degree when they are disturbed or frightened: yet their flesh is considered so savoury by the negroes that they are unwilling to sell those which they catch, and would not exchange it for any other food, except perhaps for the 'ourite,' which is the Catfish hung up in the sun until it acquires a most fœtid smell, tainting the atmosphere to a great distance; in this state it is a chief ingredient in their favourite ragout. This mode of living may be one of the causes of the peculiar odour of the skin of the woolly-headed race, which no ablutions can remove, and which is not less distinctive of their race than the colour of the skin itself."

Mr. Telfair then refers to the collection of *Fishes* last presented by him to the Society, portions of which were exhibited at the Meetings of the Committee on the 12th and 26th of April. He is continuing his ichthyological collections, and states the proceeding which he adopts in the preservation of the specimens to be as follows. "The moment the fish is caught it is thrown into a tub of rum; and the numbers are gradually augmented until there is no further room and the spirit begins to acquire a slight smell of the fish. They are then taken out; washed in fresh rum; and again put into clean spirit. They are then ticketed and numbered with lead and wire, and are ready to be put up in the preparation bottles as opportunities for their embarkation offer: this is done with fresh spirit also." The success of this method was shown to be in many instances almost complete, the fishes exhibiting great beauty and brilliancy of colour. In some cases, however, it is less successful, and even the same species varies considerably in its state of preservation. Thus of the *Julis decussatus*, (*Sparus decussatus*, J.W. Benn.) two specimens almost equal the brilliancy depicted in the 'Fishes of Ceylon' [Plate xiv.], while a third has parted with nearly the whole of its colouring, and retains merely the markings. The iron wire employed in affixing the leaden numbers has generally rusted so as to stain the fishes where it has been in contact with them, and has in some instances been so weakened by corrosion as no longer to retain the lead.

Mr. Telfair concludes by referring to the neighbouring island of Madagascar, and to the interest attaching to its natural productions so far as they have been already investigated. He remarks how imperfect this investigation yet is, and gives a historical sketch of the various attempts made by European naturalists during the last twenty years, but few of which have been attended with even moderate

rate

rate success. In several instances they have been fatal to the zealous individuals who have devoted themselves to the pursuit, the climate, especially that of the coast, being generally ill suited to Europeans. A new attempt is about to be made under the auspices of Mr. Telfair and the Mauritius Natural History Society, from which he anticipates considerable additions to science, the individual selected being well adapted for the purpose by long practice in collecting and preserving specimens, and by being thoroughly acclimated to Madagascar, in which he has on several occasions resided for a considerable time.

Mr. Owen, having had occasion to examine recently with Mr. Yarrell the body of a *Gannet*, (*Sula Bassana*,) which died at the Society's Garden, read his notes of the examination. They referred chiefly to the situation and connections of the air-cells, and differed in some particulars from the observations recorded by Montagu, who states in the 'Supplement to the Ornithological Dictionary' [article *Gannet*], that "by reason of some valvular contrivance the skin could not be artificially inflated through the lungs;" and adds, "it is also clear that there is no direct communication between the sides."

"In the examination our attention was chiefly directed to the air-cells, which in this bird, as in the *Pelican*, have a most extensive distribution. We commenced by gentle but continued inflation through the *trachea*, a pipe having been introduced into the upper *larynx:* in a short time the integuments of the whole of the lateral and inferior parts of the body rose, and the air-cells seemed completely filled, especially that which is situated in front of the *os furciforme*. Being thus satisfied that they all had a free communication with the chest, we next proceeded to see at what points these communications took place, and in what degree the air-cells communicated with each other. For that purpose the air-cells on the left side of the body were laid open, and shortly after those of the opposite side collapsed, indicating the existence of apertures of communication, although the *septum* which ran along the middle line of the body appeared at first sight imperforate. There was a free communication between the lateral air-cells of the same side of the body from the *os furciforme* to the side of the *pelvis;* but the air-cell in front of the *os furciforme* remained still tensely inflated. The lateral air-cells had a free communication with the cavity of the chest at the *axilla*, at which part the air had entered these cells during the inflation. The pectoral muscles and those of the thigh presented a singular appearance, being as it were cleanly dissected, having the air-cells extended above and below them; the axillary vessels and nerves also passing bare and unsupported by any surrounding substance through these cavities. We traced the air-cells down the side of the *humerus*, *ulna*, and metacarpal bone, into all of which the air entered, and even into the bone corresponding to the first *phalanx*, which agrees with what Mr. Hunter has described in the *Pelican*. (Animal Œcon. p. 92.)

"As none of these proceedings had any effect on the air-cell in front of the *os furciforme*, which still continued distended, it was evident

evident that inflation by the *humerus* could not have filled it except through the medium of the lungs themselves. We next proceeded to detach the integument from this air-cell to see its shape and extent; this required to be done with great care, as it adhered pretty closely to the skin and roots of the feathers: it was of a globular form, about four inches in diameter, and communicated with the *thorax* at its anterior aperture below the *trachea*.

"Numerous strips of muscular fibres passed from various parts of the surface of the body, and were firmly attached to the skin; a beautiful fan-shaped muscle was also spread over the external surface of the air-cell anterior to the *os furciforme*. The use of these muscles appeared to be, to produce instantaneous expulsion of the air from these external cells, and by thus increasing the specific gravity of the bird to enable it to descend with the rapidity necessary to the capture of a living prey while swimming near the surface of the water.

"With respect to the general anatomy of this bird, it may be observed that we found the two small glands at the termination of the *trachea*, which are noticed by Montagu, and which exist in addition to the ordinary pair lying above the *bronchiæ*. The stomach corresponded exactly with the figure given by Sir Everard Home (Comp. Anat. pl. xlvi.), the pyloric orifice being provided with the bilobed valve which is there represented, though not described in the text; it evidently opposes a too ready egress of the contents of the stomach."

Mr. Vigors exhibited a collection of African Birds which had been presented to the Society by Henry Ellis, Esq., of Portland Place. They consisted of about one hundred and thirty species, many of them of extreme rarity and value, and a great portion unknown to the cabinets of England. They came immediately from Algoa Bay; but were supposed to have been collected far in the interior of the country. Mr. Vigors expressed his intention of laying before the Committee at an early Meeting, a descriptive catalogue of the whole collection, as well as whatever particulars he could collect respecting the locality from which it was brought. He named and characterized in the mean time the following apparent novelties from the *Insessorial Birds*.

TURDUS GUTTATUS. *Turd. supernè olivascenti-brunneus, subtùs subrufescenti-albidus; strigis tribus genarum, guttis rotundis pectoris abdominisque, tectricumque alarum notis brunnescenti-atris; tectricibus alarum, rectricibusque tribus utrinque lateralibus ad apicem albo notatis.*

Statura paulo minor quàm *Turdi iliaci*, Linn.

PYRRHULA ALBIFRONS. *Pyrr. nigra, capite nuchâque ferrugineo nitore subtinctis; fronte maculâque remigum albis.*

Longitudo *corporis*, $7\frac{3}{4}$; *alæ*, 4; *caudæ*, 3; *tarsi*, $\frac{3}{4}$; *rostri*, $\frac{3}{4}$, altitudo $\frac{3}{4}$.

PLOCEUS GUTTURALIS. *Ploc. suprà pallidè olivaceo-brunneus; capite colloque in fronte aurantiacis, corpore subtus aurantiaco-flavo; gulâ juguloque nigris, rostro attenuatiore.*

Longitudo *corporis*, $6\frac{1}{2}$.

PLOCEUS

PLOCEUS SPILONOTUS. *Ploc. capite suprà corporeque subtùs aurantiaco-flavis; gulâ, jugulo, dorsoque summo nigris, hoc flavo maculato; uropygio fusco-lutescente; alis caudâque fuscis.*

Statura præcedentis; rostro fortiore.

PLOCEUS CHRYSOGASTER. *Ploc. capite genis corporeque toto suprà saturatè castaneo-brunneis; gulâ flavo et brunneo variegatâ; corpore subtùs aureo-flavo.*

Statura præcedentium; at rostrum multò validius.

LAMPROMORPHA* CHALCOPEPLA. Mas. *Lamp. suprà splendidè viridis, cupreo nitens; subtùs alba, lateribus viridi-cupreo fasciatis; strigâ in capitis medio, secundâ superciliari, alterâque maxillari, maculis tectricum alarum, remigum, rectricumque, duabus mediis exceptis, albis.*

Fœm. aut mas jun.? *Lamp. corpore suprà metallicè viridi; capite, nuchâ, regioneque interscapulari cupreo splendentibus; collo in fronte pectoreque rufescenti; abdomine albo, lateribus viridi-æneo fasciatis; caudâ ferrugineâ, viridi-æneo fasciatâ; rectricum trium utrinque lateralium pogoniis, omniumque apicibus albo notatis.*

Statura *Cuculi aurati*, Gmel.

CORYTHAIX PORPHYREOLOPHA. *Cor. collo, abdomine medio, pectore, regioneque scapulari gramineo-viridibus, his subrufescentibus; fronte strigâque per oculos splendidè viridibus; capite cristato, alis, caudâque splendenti-purpureis; remigum fasciâ latâ subpurpurascenti-coccineis; dorso abdomineque imis, tectricibusque femorum fusco atris; rostro pedibusque atris.*

Statura *Cor. Persæ*, Ill.

BUCCO NANUS. *Bucco suprà niger, sulphureo striatus; strigâ superciliari gracili, alterâque per totam longitudinem alarum extendente latâ, aurantiis; gulâ crissoque sulphureis, abdomine fuscescenti; fronte coccineo.*

Longitudo *corporis*, $4\frac{1}{4}$; *rostri* ad frontem, $\frac{7}{16}$, ad rictum $\frac{9}{16}$.

YUNX PECTORALIS. *Y. suprà pallidè brunnescenti-griseus, fusco graciliter undulatus; nuchâ scapularibusque nigro notatis, caudâ nigro fasciatâ; subtùs albidus, collo in fronte confertim, femorum tectricibus minùs confertim, nigro fasciatis, abdomine nigro lineato; maculâ grandi pectorali ad gulam extendente rufâ; remigibus fuscis, pogoniis externis ferrugineo fasciatis.*

Statura *Y. Torquillæ*, Linn.

June 28, 1831. Rev. W. Kirby in the Chair.

A letter from Sir Robert Ker Porter, Corr. Memb. Z. S., dated City of Caraccas, Venezuela, March 25, 1831, was read. It announced his having recently obtained possession of a specimen of the *American Tapir*, (*Tapir Americanus*, Gmel.), which it was his

* A group including the shining *Cuckoos* of Africa, India, and New Holland, indicated in the *Transactions of the Linnean Society*, vol. xv. p. 300. Mr. Vigors expressed his belief of having lately seen a name attached to this group by some modern author; but he could not call to his recollection the work in which it occurred.

 intention

intention to transmit to the Society at the earliest opportunity. It embraced a full description of the animal; and entered at considerable length into an account of its habits. The letter was accompanied by two drawings of the *Tapir,* and by sketches of its proboscis-like upper lip.

Mr. Gray exhibited the skins and skulls of two *Mammalia* brought from China by Mr. Reeves, together with the skull of a third, of which a skin was also in his possession. On these he proposed to found three new genera, the characters of which may be given as follows:

Helictis.

Dentes primores $\frac{6}{6}$: *laniarii* $\frac{1}{1}\,\frac{1}{1}$: *molares* $\frac{5}{6}\,\frac{5}{6}$; *e quibus* $\frac{3}{4}\,\frac{3}{4}$ *anteriores falsi conici compressi; carnivori* $\frac{1}{1}\,\frac{1}{1}$, *in maxillâ superiori 3-lobati, cum processu interno subcentrali lato 2-acuminato; tuberculares* $\frac{1}{1}\,\frac{1}{1}$, *superiores mediocres transversi, inferiores exigui. Caput elongatum. Pedes breves; plantæ ad calcaneum ferè nudæ; digiti 5—5; ungues validæ, anteriores longæ compressæ. Cauda cylindrica mediocris.*

This genus, which inhabits eastern Asia, has the general appearance and colouring of *Mydaüs,* combined with a dentition resembling that of *Gulo* or *Mustela,* but differing from both the latter genera in the large internal central lobe of the upper carnivorous tooth. The species exhibited may be characterized in the following terms:

Helictis moschata. *Hel. suprà argentata, pilis singulis basi cinereis apice argenteo-albis, colore argenteo ad latera corporis et versus apicem caudæ dominante, capite pedibusque anticis in fusco-cinerascentem vergentibus; strigâ inter, aliisque duabus pone, oculos, maculâ interauriculari nuchalique, labio superiore, mento, gulâ, gastræo medio, femoribusque internis, albis.*

The entire length of the animal is $23\frac{1}{2}$ inches, of which the tail measures 8. It inhabits China, and smells strongly of musk.

Mr. Gray added that the *Gulo orientalis* of Dr. Horsfield's 'Zoological Researches in Java' appeared to him to form a second species of the genus, closely resembling the Chinese in its general characters, and in the disposition of its colouring, but differing in its browner colour and in the larger proportion of white upon the head and back. The internal lobe of the upper carnivorous tooth in the Javanese animal is also described as being anterior and very minute.

Paguma.

Dentes primores $\frac{6}{6}$ *æquales: laniarii* $\frac{1}{1}\,\frac{1}{1}$: *molares* $\frac{6}{6}\,\frac{6}{6}$; *quorum utrinque in maxillâ superiori 3 falsi parvi compressi, 1 carnivorus brevis obtusè 3-lobus cum processu interno centrali, 2 tuberculares subquadrati釜 internè subangustati anticè non producti; in maxillâ inferiore 4 falsi, 1 carnivorus, 1 tubercularis. Pedes postici plantigradi, ad calcaneum usque nudi callosi. Cauda longa attenuata.*

In the number and disposition of its teeth this genus agrees with *Viverra,* from which, however, it differs in their conformation. It is much like *Ictides* in colouring, but has about the face the pale marking

marking of *Paradoxurus:* the skin has the odour of civet. From the genus *Viverra* it is distinguished by the shape of its skull, the cerebral cavity being in it much larger, the space between the eyes broader, and the nose much broader and shorter. The species was characterized in the following terms:

PAGUMA LARVATA. *Pag. grisea; fasciâ albâ frontali transversâ, alterâque longitudinali per frontem ad nasum ductâ; caudâ apice nigrescenti.*

Gulo larvatus. *Ham. Smith in Griff. Transl. Cuv. Règn. An.* ii. p. 281, c. fig.

Viverra larvata. *Gray, Spic. Zool.* p. 9.

The third genus described was founded on a *glirine* quadruped, nearly allied to the Bamboo-Rat (*Mus Sumatrensis,* Raffl.?), with which Mr. Gray associated it under the following characters.

RHIZOMYS.

Dentes primores $\frac{2}{2}$ maximi, elongati, triangulares, acutati: molares $\frac{3}{3}$ $\frac{3}{3}$ radicati, subcylindrici, coronis transversim subparallelim porcatis; superiores internè lobati. Caput magnum. Oculi parvi aperti. Auriculæ nudæ conspicuæ. Corpus crassum subcylindricum. Pedes breves validi, digitis 5—5. Cauda mediocris, crassa, nuda.

In teeth and general appearance this genus is most nearly allied to *Spalax,* from which it differs in its tail of moderate length, its exposed eyes and ears, and the more complex character of its molar teeth. The species of *Rhizomys* live moreover upon, and not under, the ground, being found about Bamboo-hedges, on the roots of which they principally subsist. The following were stated to be the distinctive characters of the two species known.

RHIZOMYS SINENSIS. *Rhiz. pallidè cinerascens unicolor.*

Hab. in Chinâ. *D. Reeves.*

RHIZOMYS SUMATRENSIS. *Pallidè fuscus, pilis raris albidis interspersis; corporis lateribus pedibusque saturatioribus; genis pallidioribus, occipite nigrescenti lineâ longitudinali albâ, pectore albido.*

Mus Sumatrensis, *Raffles, Linn. Trans.* xiii. 258? *Temminck, Mus. Leyd.*

Spalax Javanus, *Cuv. Règne Anim.*, ed. 2., i. 211.

Hab. in Sumatrâ, *Raffles? Temminck;* Javâ, *Cuvier.*

The latter species seems to have been first observed by Colonel Farquhar, in whose collection of drawings, preserved in the Museum of the Asiatic Society, a representation of it is found. Of the former we owe the discovery to Mr. Reeves.

XXXI. *Intelligence and Miscellaneous Articles.*

PREPARATION OF IODIC ACID. BY ARTHUR CONNELL, A.M.

THE methods which have been hitherto followed for the formation of iodic acid, may be reduced, Mr. Connell remarks, to three: *first,* the action of alkaline solutions, giving rise to the formation of a hydrio-

a hydriodate and an iodate, from the latter of which the iodic acid may be separated by the original method of M. Gay-Lussac, and more perfectly by the recent method of M. Serullas (*Ann. de Chim. et de Phys.* xliii. pp. 127 and 217); *secondly*, the action of euchlorine, as suggested by Sir H. Davy; and, *thirdly*, the action of water on the perchloride of iodine, and subsequent separation of iodic acid by means of alcohol, as also proposed by M. Serullas (see Phil. Mag. and Annals, N.S, vol. ix. p. 149): to these Mr. Connell proposes to add the agency of nitric acid, which he thinks will be found to equal in facility of execution any of the preceding processes.

The vessel employed was a rather large and tall flask, into which fifty grains of iodine and an ounce of fuming nitric acid were put; the acid was made to boil, and as soon as any iodine sublimed and condensed on the sides of the vessel, it was washed back again into the liquid by agitation. After the process had been continued some time, a precipitation of white crystalline grains was observed to take place; and the operation of boiling and washing back the sublimed iodine was continued until the free iodine had to a great extent disappeared. The whole was then decanted into a shallow basin and evaporated to dryness. Any free iodine which had remained was soon dissipated by the heat. The residue of the evaporation consisted of whitish crystalline grains, which were iodic acid, retaining a little nitric acid, from which they appeared to be freed by one or two solutions in water, and re-evaporations, when they lost most of their crystalline appearance, and became a whitish deliquescent mass, occasionally with a light purplish tint, from a tendency to decomposition by the heat of evaporation. Where no particular precautions were taken to prevent loss in the state of vapour, and where the process was not continued until the entire disappearance of iodine, the quantity of acid obtained approached that of the iodine employed; a larger proportion of iodine might probably be used, with the same quantity of acid.—*Jameson's Journal*, June 1831, p. 72.

NEW SCIENTIFIC BOOKS.

Just published.

The Twenty-second Number of Professor Leybourn's Mathematical Repository is published. It contains solutions to twenty questions in different parts of pure and mixed mathematics (and as many new ones for future solution) by various contributors.

The separate papers are: 1. Two indeterminate Problems, by James Cunliffe, Esq. late of the Roy. Mil. Coll. 2. Analysis and Construction of a Geometrical Problem, by C. F. Barnwell, Esq., A.M. F.R.S. F.S.A. 3. A History of the Investigations respecting the Properties of Rule Surfaces, or such as can be generated by the motion of a right line subjected to certain conditions, by T. S. Davies, Esq. F.R.S.E. F.R.A.S. 4. An Inquiry into the Author of the Second and Third Properties of the Stereographic Projection of the Sphere, completing the Inquiries of Delambre on that Subject in his History of Astronomy, by the same Gentleman. 5. *Horæ Arithmeticæ*,

Arithmeticæ (No. 8.); or Historic Memoranda respecting circumstances connected with his new method of Continuous Approximation to the Roots of Numerical Equations, by W. G. Horner, Esq. 6. Analytical Investigation of the Curious Property of Lines of the second order which formed the Prize Question of this number (geometrical demonstrations of which had been given in their proper place by Messrs. Davies and Woolhouse), by T. S. Davies, Esq. F.R.S.E. &c. 7. Researches in the Geometry of Three Dimensions (incomplete), by the same Gentleman. 8. Pascal's first work, being on Conics, together with an account of other papers of his on the same subject, by Leibnitz. 9. Solutions of the Sixty Problems in the Rev. John Lawson's Geometrical Analysis of the Ancients, by the Rev. Charles Wildbore.

The Number also contains lists of the mathematical papers published in the Transactions of different learned Societies; and of the Questions proposed in the Cambridge Senate House on examination for degree of A.B. and for Smith's prizes.

MR. SAULL'S GEOLOGICAL MUSEUM.

W. D. Saull, F.G.S. &c. has recently become the possessor of the extensive Geological Museum of the late Mr. Sowerby, of Mead Place, Lambeth, the whole of which is now stratigraphically arranged, with the addition of Mr. Saull's previous collection of fossils, and will be open for the inspection of scientific gentlemen, and friends, every Thursday morning, at his residence No. 15, Aldersgate-street, City.

LUNAR OCCULTATIONS FOR SEPTEMBER.

Occultations of Planets and fixed Stars by the Moon, in September 1831. Computed for Greenwich, by THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1831.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean solar time.	Immersions. Angle from North Pole.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean solar time.	Emersions. Angle from North Pole.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Sept. 11	γ Libræ...	4·5	1764	20 7	8 46	84	120	Under	horizon.	...	...
19	58 Aquarii	6	2690	2 27	14 34	141	174	3 22	15 29	263	301
20	χ Aquarii	5·6	2776	22 3	10 7	122	110	23 17	11 21	288	290
22	33 Ceti.....	6	125	3 5	15 0	130	152	4 10	16 5	278	309
30	o^2 Cancri...	6	1094	4 7	15 30	69	29	5 11	16 34	275	238

METEORO-

METEOROLOGICAL OBSERVATIONS FOR JULY 1831.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·348. July 6. Wind N.E.—Min. 29·606. July 13. Wind S.E.
Range of the mercury 0·742.
Mean barometrical pressure for the month .... 29·999
Spaces described by the rising and falling of the mercury........... 3·190
Greatest variation in 24 hours 0·259.—Number of changes 15.
Therm. Max. 79°. July 9. Wind W.—Min. 52°. July 3. Wind W.
Range 27°.—Mean temp. of exter. air 64°·68. For 31 days with ☉ in ♋ 63·40
Max. var. in 24 hours 22°·00.—Mean temp. of spring-water at 8 A.M. 51·81

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 23rd.... 100°
Greatest dryness of the atmosphere, in the afternoon of the 9th ... 43·0
Range of the index .... 57·0
Mean at 2 P.M. 55°·2.—Mean at 8 A.M. 61°4.— Mean at 8 P.M. 67·5
—— of three observations each day at 8, 2, and 8 o'clock .... 61·4
Evaporation for the month 4·55 inches.
Rain in the pluviameter near the ground 3·465 inch.
Prevailing winds, West.

Summary of the Weather.

A clear sky, 4½; fine, with various modifications of clouds, 15½; an overcast sky without rain, 7½; rain, 3½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
23	16	27	3	28	23	16

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	4	1¼	6	1¼	6	8	2½	31

General Observations.—This month has been fine, with the exception of four or five days, when much rain fell here. In the evening of the 5th, Venus was in conjunction with α Leonis, and they formed a small isosceles triangle with Saturn. At midnight of the 8th several flashes of lightning ascended from the horizon in the north-east quarter. The 9th was the hottest day in the shade, and there were a few flashes of lightning in the night in the south-east horizon. Sheet lightning also occurred the following evening. Much rain fell on the 11th, 12th, and 14th, which was accompanied with lightning and thunder. Distant thunder and lightning also occurred in the afternoons of the 16th and 28th. In the afternoon of the latter day the edge of a thunder-storm passed this place, and proceeded with some violence in its electrical effects in the direction of Berkshire. Not only in Hampshire, but in most other counties, thunder-storms have often occurred through the month, so that the period may be said to have been *a war of the elements.*

On the 19th, 20th, 21st, and 22nd, a hard gale blew from the South-west, and on the following day it blew equally hard from the opposite point of the compass, with a copious rain, and a considerable decrease in the temperature of the air.

The rain and wind lodged some of the corn in this neighbourhood.

From the 25th to the end of the month the weather was dry and warm, and on several days a thermometer in the sun's rays rose to 120 degrees, which had the effect of ripening the wheat, and harvest commenced here at the close with every prospect of good average crops.

Soon after ten o'clock in the night of the 30th, an aurora borealis appeared, whose upper arch, though not well defined, was about 16 degrees high

high in the magnetic north; but the lower arch was low, and could not be traced in consequence of a dark vaporous horizontal cloud before it. At a quarter past ten several bright yellow columns, 20° in altitude, rose from the aurora under Polaris, when one brilliant meteor descended between that star and Dubhe in Ursa Major. Coruscations continued to rise between the true and magnetic north till a quarter to eleven, when the moon was several degrees above the horizon, and the aurora disappearing. It was lately mentioned at Portsmouth by a public lecturer on aërology, that Jupiter's attraction of the atmosphere of the earth is greater than has been generally supposed; and that when the moon is near him, he has so powerful an attraction over our atmosphere, as to disturb its elasticity and draw it up considerably out of its spheroidal form, by which means electrical action and a condensation are produced, *so as to cause heavy rain;* and that the effect is greater when Jupiter crosses the northern part of the Pacific Ocean in his north declination, and the South Sea and Atlantic Ocean in his south declination, in consequence of the abundance of vapours arising from them. This opinion (conceived to be new by the lecturer), although not new to men of scientific pursuits, certainly deserves the strictest investigation in a meteorological point of view, it having been verified in almost all the lunations this year, even in this latitude, when the moon has been near Jupiter; and as this planet is 1312 times larger than the earth, it is not surprising if we admit the principle of attraction of the heavenly bodies, that he should conjointly with the moon exert so great an influence over our atmosphere.

The mean temperature of the external air this month is nearly half a degree higher than the mean of July for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are, two solar halos; one meteor; one aurora borealis; lightning on eight days and thunder on four, and five gales of wind, namely, one from the North-east, three from the South-west, and one from the West.

REMARKS.

London.—July 1, 2. Fine. 3. Slight rain: fine. 4. Very fine. 5. Fine: slight rain at night. 6—9. Very hot. 10. Thunder at noon, with slight rain: very heavy storm to the eastward. 11. Hot. 12. Heavy showers. 13. Rain in the morning: fine. 14. Cloudy: rain at night. 15. Rain, with some thunder in the afternoon. 16. Cloudy, with thunder showers. 17—19. Fine. 20. Cloudy and windy, with slight rain. 21. Fine. 22. Fine, with slight showers. 23. Fine: rain at night. 24—27. Very fine and warm. 28. Sultry; thunder, with rain towards night. 29. Very hot: thunder in the afternoon. 30, 31. Very fine.

Penzance.—July 1. Fair. 2. Fair: rain. 3. Fair. 4. Misty: fair. 5. Rain: fair. 6. Clear. 7. Fair: clear. 8. Clear. 9, 10. Fair. 11. Fair: rain. 12. Fair: thunder-shower. 13. Showers. 14. Fair: showers. 15. Fair: rain. 16. Fair: shower. 17. Fair. 18. Showers: fair. 19. Misty: rain. 20. Rain: showers. 21, 22. Showers. 23. Heavy rain. 24—27. Clear. 28. Fair. 29—31. Clear.

Boston. — July 1, 2. Fine. 3. Cloudy. 4. Fine: Therm. 74° 3 P.M. 5. Cloudy: rain A.M. 6. Cloudy. 7, 8. Fine. 9. Fine: Therm. 78·5, 1 P.M. 10. Cloudy: rain P.M. 11. Cloudy. 12. Fine. 13. Cloudy: rain A.M. and P.M. 14. Fine: rain, with heavy thunder-storm 1 P.M. 15, 16. Rain. 17—19. Fine. 20. Cloudy. 21. Cloudy: rain early A.M. 22. Stormy. 23—25. Fine. 26. Cloudy. 27. Fine: Therm. 74° 4 P.M. 28. Cloudy. 29. Fine: Therm. 80° 1 P.M. 30. Cloudy: rain early A.M. 31. Fine.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1831.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
July 1	30·097	30·055	30·10	30·10	30·123	30·088	29·51	73	47	63	54	71	53	60	NW.	NW.	W.	NE.	...	...	...	...	...
☾ 2	30·025	29·994	30·02	30·00	30·093	30·053	29·47	78	55	64	50	69	59	58·5	NW.	W.	W.	N.	...	...	0·210	0·030	...
3	30·080	30·010	30·10	30·00	30·125	30·040	29·41	78	51	65	54	71	52	64	W.	NW.	W.	calm	0·45	0·01	...	...	...
4	30·204	30·171	30·08	30·08	30·257	30·238	29·60	80	51	70	52	72	59	64	W.	SW.	W.	NW.	...	...	...	...	...
5	30·255	30·124	30·18	30·08	30·257	30·238	29·55	80	53	64	59	72	59	62·5	W.	SW.	SW.	calm	...	·03	·070	·050	0·23
6	30·364	30·337	30·25	30·22	30·348	30·332	29·74	83	53	70	54	72	61	66	E.	NE.	SE.	calm	·65	...	...	...	...
7	30·331	30·266	30·20	30·20	30·293	30·240	29·80	78	50	71	53	72	57	62	E.	SE.	E.	E.	·20	...	...	...	...
8	30·240	30·190	30·15	30·15	30·242	30·193	29·65	79	49	71	56	73	57	63·5	E.	SE.	N.	NW.	·30	...	...	...	...
● 9	30·161	30·103	30·15	30·10	30·206	30·118	29·52	86	57	66	57	79	60	69·5	SW.	NW.	NW.	NW.	...	...	...	...	...
10	30·049	30·009	30·05	30·00	30·038	29·985	29·40	73	50	66	56	72	59	65	E.	NW.	W.	calm	...	·01	...	...	...
11	29·885	29·729	29·90	29·85	29·902	29·775	29·25	79	58	65	56	73	58	61	E.	W.	SE.	NW.	·30	·01	·120	·470	20
12	29·674	29·646	29·60	29·60	29·666	29·641	29·03	76	54	63	50	71	56	67	SW.	NE.	SE.	E.	...	1·10	·200	·910	...
13	29·707	29·584	29·60	29·60	29·681	29·606	29·07	74	51	64	54	68	56	65·5	W.	SE.	SE.	E.	...	·08	·110	·360	...
14	29·750	29·721	29·70	29·60	29·731	29·707	29·20	74	53	65	53	68	57	66	SW.	SE.	SE.	NE.	·35	·60	·095	·660	·37
15	29·785	29·735	29·70	29·70	29·798	29·760	29·12	75	54	64	52	70	59	60	SW.	NW.	SW.	NW.	...	·18	·100	·030	·50
☽ 16	29·830	29·745	29·90	29·85	29·863	29·760	29·22	73	54	66	54	70	58	59	W.	NW.	NW.	E.	...	·11	·030	·045	·68
17	30·024	29·939	30·00	30·00	30·047	29·980	29·35	79	53	66	54	72	57	61·5	W.	W.	NW.	NW.	·40	...	...	...	·02
18	29·994	29·913	30·00	30·00	30·058	29·955	29·37	73	53	66	54	70	58	64	SW.	W.	SW.	W.	...	...	...	...	...
19	29·896	29·873	29·90	29·90	29·948	29·930	29·26	73	57	66	53	71	59	62·5	SW.	W.	SW.	W.	...	...	·100	...	...
20	29·790	29·750	29·80	29·80	29·860	29·780	29·13	70	60	66	56	67	61	61·5	SW.	SW.	SW.	W.	...	·03	·155	·020	...
21	29·706	29·636	29·80	29·70	29·743	29·681	29·05	73	50	65	55	68	55	62	SW.	SW.	W.	W.	·45	...	...	·060	·19
22	29·817	29·802	29·80	29·80	29·855	29·847	29·16	70	47	65	53	68	54	62	SW.	W.	SW.	W.	...	·10	...	...	...
23	29·825	29·532	29·75	29·60	29·845	29·664	29·22	68	54	63	54	67	56	63	SW.	S.	W.	NW.	...	·14	1·205	·800	...
○ 24	29·956	29·825	29·90	29·70	29·923	29·789	29·33	70	49	65	54	68	58	64	E.	NE.	NE.	E.	·35	...	...	...	...
25	30·129	30·045	30·00	29·95	30·075	30·029	29·52	73	49	69	54	71	56	64	E.	NE.	NE.	E.	...	...	...	...	...
26	30·197	30·144	30·13	30·10	30·190	30·125	29·60	80	51	67	54	75	56	65	E.	NE.	N.	E.	...	...	...	...	...
27	30·190	30·112	30·18	30·15	30·212	30·201	29·50	85	55	71	53	73	59	65	N.	W.	W.	E.	·30	...	...	...	...
28	30·198	30·081	30·12	30·10	30·146	30·106	29·50	85	56	71	60	77	60	66·5	NW.	S.	NW.	NW.	...	·04	...	·030	...
29	30·138	30·083	30·00	30·00	30·126	30·108	29·43	85	54	70	59	75	62	68	SW.	E.	SE.	calm	...	...	...	...	...
30	30·158	30·121	30·00	30·00	30·158	30·088	29·50	83	54	74	57	75	61	68	E.	E.	NE.	calm	...	·08	...	...	·16
31	30·097	30·019	30·00	29·98	30·083	30·042	29·50	82	52	74	58	77	61	67	E.	E.	NE.	calm	·80	...	...	...	...
	30·364	29·532	30·25	29·60	30·348	29·606	29·38	86	47	74	50	79	52	63·7					4·55	2·52	2·395	3·465	2·35

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XXXII. *Researches on some of the Revolutions which have taken place on the Surface of the Globe; presenting various Examples of the Coincidence between the Elevation of Beds in certain Systems of Mountains, and the sudden Changes which have produced the Lines of Demarcation observable in certain Stages of the Sedimentary Deposits.* *By* L. ELIE DE BEAUMONT*.

TWO great views, one a succession of violent revolutions, the other the elevation of mountain-chains by forces acting from beneath, having been successively introduced into geology, it was natural to inquire if they were independent of each other; if mountain-chains could be raised without producing real revolutions on the surface of the globe; if the frightful convulsions which must have accompanied the upburst of masses so great and of an aspect so contorted as those of high mountains, were not the same with those revolutions on the surface of the globe which are proved to have taken place by the mineralogical and zoological lines of demarcation observable in the sedimentary deposits.

The principal object of the researches, of which the following is a brief sketch, is to show, term for term, the connection of these two series of facts.

It will be necessary to premise a few words respecting the principles on which these researches have been conducted. The expression *sedimentary deposits* (*terrains de sediment*) in which we, in some measure, sum up our knowledge of those masses so widely spread over the surface of our planet, so

* Extract forwarded to Mr. De la Beche in May 1831, and communicated by the latter to the Editors.

naturally carries with it the idea of *horizontality*, that it is never without surprise we first hear of sedimentary beds observed in a vertical or nearly vertical position.

As early as 1667, Stenon maintained that all inclined sedimentary beds were upraised; and since the observations of De Saussure on the Valorsine conglomerates, geologists have generally agreed in considering those sedimentary beds which are frequently observed in mountainous countries either inclined at considerable angles, placed vertically, or even thrown over, as not having been formed in that position, but as having been so circumstanced, in consequence of phænomena which have taken place at a greater or less time after their original deposition.

There are few countries where these phænomena have been produced at so late a period, as to affect all the sedimentary deposits there existing, even abstracting the alluvion of modern rivers, which in all cases has not yet been disturbed by any phænomena of this nature.

We observe along nearly all mountain chains, when we attentively examine them, that the most recent rocks extend horizontally up to the foot of such chains, as we should expect would be the case if they were deposited in seas or lakes of which these mountains have partly formed the shores; whilst the other sedimentary beds tilted up, and more or less contorted on the flanks of the mountains, rise in certain points even to their highest crests. Thus in each chain, or rather in each system of chains, the series of the sedimentary rocks is divided into two distinct classes, and the point of separation of these two classes, variable from one system to another, is one of the circumstances which best characterizes each particular system.

At the same time that the position of the ancient and inclined beds furnishes the best proof of the elevation of the mountains of which they constitute a part, the geological age of these beds affords the best means of determining the relative age of the mountains themselves; for it is evident, the first appearance of the chain itself is necessarily intermediate between the period when the beds, now upraised, were deposited, and that when the strata were produced horizontally at its feet.

There is nothing so essential to remark, as the constant clear line of separation between these two series of beds in each chain. This kind of observation is sanctioned by long experience. Geologists have, in fact, been long accustomed to employ the absence of parallelism in the stratification of two systems of beds, the one supporting the other, as affording the

the clearest line of demarcation that can be found between two systems of consecutive sedimentary deposits. This idea, which has been developed in the lessons of the most distinguished professors, has, it may be said, become common. It was indeed on a fact of this nature, generalized certainly beyond measure, that Werner founded his principal division in the series of rocks. Now it follows from this difference, always clear and without passage, between the upheaved beds and those which are horizontal, that the elevation of the beds has not been effected in a continuous and progressive manner, but that it has been produced in a space of time comprised between the periods of deposition of the two consecutive rocks, and during which no regular series of beds was produced;—in a word, that it was sudden, and of short duration.

It has been in vain attempted to explain the geological facts observable in high mountain-chains, by the action of the slow and continuous causes now in force on the surface of the globe. No satisfactory result has been obtained by these means. In fact, everything shows that the instantaneous elevation of the beds of a whole mountain-chain is an event of a different order from those which we daily witness. It is evident that such a convulsion would interrupt the slow and progressive formation of the sedimentary deposits, and that some anomalous circumstance would be nearly universally observable in that point of the series of rocks which should correspond with the moment when an elevation of beds took place. It is well known that those geologists who have most carefully examined the sedimentary deposits, and those naturalists who have investigated the remains of animals and vegetables which they contain, have generally remarked that between different terms of the series of these rocks there are sudden variations, not only in the position and local character of the beds, but also in the fossil animals and vegetables entombed in them. From observations which did not comprise a sufficiently extensive area, some of these variations (to lessen the value of which too many attempts have perhaps been subsequently made) were at first supposed more general than they really are. When two formations appear to pass insensibly into each other, there is never more than a small depth of beds of which the classification may remain uncertain; and when certain fossils are common to two successive formations, they generally constitute a fraction, often even inconsiderable, of the total number of species found in each of the two formations. This is more particularly seen in the comparison instituted by M. Deshayes (in a work impatiently expected by geologists) between the catalogues of the species of shells

discovered in the three groups, which he distinguishes in the beds above the chalk, and the catalogue of species now existing. It is sufficient, that in the series of superimposed beds there are points more remarkable than others, on account of the changes they exhibit, both in the deposits and in the inhabitants of the same country, to be struck with the accordance of the two orders of considerations above noticed.

Among those observations which render it impossible to consider the dislocation of beds which characterizes a mountainous country as the result of local phænomena, which may have been repeated in an irregular and successive manner, we may place in the first rank the constancy of the direction in which sedimentary beds are tilted up even for immense distances.

Practice has taught miners from time immemorial the principle of constancy in directions, and it is one of those circumstances which they most usefully employ in their researches. The observation of constancy in the direction of beds in the coal-measures, has served to discover a bed of coal at a distance, though invisible on the surface. It was by combining the observations made in numerous metallic mines, that Werner arrived at the conclusion, that, in the same district, all the veins of the same nature were due to cracks parallel to each other, formed at the same time, and subsequently filled at the same period. The remarkable phænomenon of constancy of direction has been gradually shown to be more important, by the labours of those geologists who since De Saussure and Pallas have attentively examined mountain chains. It has been admitted by degrees, that the circumstance which best characterizes mountain-chains, when compared with each other, is the direction which the elevation of the beds has impressed upon them,—a direction naturally observable in the crests composed of such beds. For more than thirty years M. Humboldt has pointed out the equally remarkable accordances and discrepancies observable in the direction of mountain-chains, whether close to, or remote from, each other. M. von Buch has also shown that the mountains of Germany are divisible into at least four systems, clearly distinguishable from each other by their directions. So clear a mode of distinction even led him to conceive that the various mountain systems were produced by phænomena independent of each other; and it is at the same time very probable, that not only, as is proved by observation, all beds upheaved at the same time have been so raised in the same direction, but also that this constancy in the direction of the upraised beds in a certain assemblage of mountains, is the result of this collection

of

of beds having been thrown up at the same time by a single effort of nature: whence it would follow that the number of the epochs of elevation would not be unlimited, but that it would at least be equal to that of the directions of those chains which are clearly distinct,—a number not incompatible with that of the solutions of continuity observable in the sedimentary deposits.

It became necessary, in order to carry the subject beyond these vague and general views, that a comparison should be instituted between the number of those lines of demarcation observable in the sedimentary deposits, and the same number of mountain systems. It has been attempted to accomplish this by combining the two great principles above noticed; namely, that the highly inclined sedimentary beds are upheaved strata, and that in each mountainous district all the beds upheaved at the same moment have been so raised in the same general direction.

The examination of the surface of Europe has in this manner already led to the determination, both with respect to age and direction, of the twelve systems of mountains to be successively noticed in the sequel, as also their relation to twelve solutions of continuity observed in the series of sedimentary deposits.

I. *System of Westmoreland and of the Hundsruck.*—The correspondence of this nature which may be referred to the most ancient geological epoch has been made known by the researches of Professor Sedgwick, recently communicated to the Geological Society of London. The mean line of bearing of the different systems of slate rocks in the lake mountains of Westmoreland, is shown by this author to be nearly N.E. by E., and S.W. by W. This causes them to abut successively against the carboniferous zone; from which it follows that they must also be unconformable to it. Professor Sedgwick strengthens this inference by reference to detailed sections: and from the whole of the evidence he concludes, that the central lake mountains were placed in their present position, not by a long continued, but by a sudden movement of elevation, before or during the period of the old red sandstone*.

Professor Sedgwick has also shown that if lines be drawn in the principal bearing of the following chains,—viz. the southern chain of Scotland, from St. Abbs Head to the Mull of Galloway; the grauwacké chain of the Isle of Man; the slate ranges of the Isle of Anglesea; the principal grauwacké

* From other circumstances to be noticed in the sequel, it appears very probable that this movement of elevation was anterior to the deposition of the most recent strata of the transition series.

chains

chains of Wales, and the Cornish chain,—they will be nearly parallel to each other and to the line of bearing of the lake mountains. The elevation of these chains, which produce marked effects on the physical character of Great Britain, is referred by Professor Sedgwick to the same period; and the parallelism is not considered accidental, but as offering a confirmation of the general principle,—that mountain-chains, all elevated at the same period of time, present a general parallelism in the bearing of their component strata.

The surface of continental Europe presents many mountainous countries, in which the predominant direction of the most ancient and disturbed beds is, as has been remarked for more than thirty years by M. Humboldt, but slightly removed from a N.E. and S.W. line. Such is, for example, the direction of the grauwacké and slate beds in the mountains of the Eiffel, the Hundsruck, and of Nassau, at the feet of which were probably deposited the coal-measures of Belgium and Saarbruck. Such is also the direction of the slate, grauwacké, and transition limestone beds of the northern and central parts of the Vosges, on the edges of which there are several small coal basins.

The parallelism of this direction to that observed by Professor Sedgwick in England, added to the fact, that in the Vosges this direction of the slate and grauwacké strata is not carried into the coal-measures, leads us naturally to suppose that the inclined position of these parallel beds of England and the Continent is due to the same catastrophe, the most ancient of any of which traces can at present be clearly recognised.

Further researches may perhaps show the relation that may exist between the different parts of the Westmoreland slate rocks, and more effaced and older elevations of strata than this now under consideration.

II. *System of the Ballons* (*Vosges*) *and of the Hills of the Bocage* (*Calvados*).—The observations noticed in the preceding article, only prove that the system of Westmoreland and the Hundsruck have been elevated before the deposition of the carboniferous series; but it would appear that it had been elevated even before the deposit of the more recent transition rocks. In fact, among those beds which we are in the habit of comprising in the general denomination of transition rocks, there is a widely extended class which has not been affected by the N.E. and S.W. elevation of the ancient slates, and which may have been deposited on these beds, previously upheaved. Such are the marly and arenaceous limestones with *Orthoceratites*, *Trilobites*, *Hysterolites*, &c. which occur in Podolia, in the environs of St. Petersburg, in Sweden, and in Norway, where

where they are in general but slightly removed from their original horizontal position. Such are also the transition rocks, so rich in organic remains, of Dudley and Gloucestershire, which appear to have been deposited at the foot of the previously elevated mountains of Wales, and which are themselves only affected by dislocations of a more recent date.

Such would also appear to be a part of the transition beds of Southern Ireland, known by the recent researches of Mr. Weaver. This distinguished geologist remarks that some parts of the system resemble, both in mineralogical and zoological characters, the rocks of Tortworth in Gloucestershire. The principal rock masses in the South of Ireland are composed of grauwacké, quartz rock, and limestone; they contain crinoidal remains, *Trilobites*, *Orthoceratites*, *Ellipsolites*, *Ammonites*, *Euomphalites*, *Turbinites*, *Neritites*, *Melanites*, and several species of *Terebratula*, *Spirifer*, *Producta*, and other bivalves, *Hysterolites*, and many genera of *Polyparia*. The anthracite and accompanying pyritiferous strata are charged with the remains or impressions of plants, belonging chiefly to the genera *Equisetum* and *Calamites*, with traces of *Fucoides*.

The transition rocks of the Bocage (Calvados) and the interior of Britanny bear a great resemblance to those described by Mr. Weaver in the South of Ireland. They are like them composed of numerous beds of slate, grauwacké, quartz rock, and limestone, containing fossils of the same class, and presenting mines of anthracite.

Finally, I am induced to refer to the same epoch the slate and grauwacké rocks with anthracite (worked for profitable purposes, and which contain vegetable impressions differing but little from those discovered in the coal-measures), which form the S.E. angle of the Vosges, and which appear to rest against the granitic masses of the environs of Gerarmer, Remiremont, and Tillot; masses which probably were themselves raised at the formation of the old N.E. and S.W. lines of elevated strata. Independently of the geological relations which are apparent between the different parts of the vast deposit of transition rocks above noticed, they have also in common remained unaffected by the ancient N.E. and S.W. system.

When these beds are not horizontal, they are dislocated in directions the most marked of which, probably produced immediately after their deposit, is comprised between an E. and W. line and one E. 15° S. and W. 15° N. Thus the masses of granite and porphyry which, in the S.E. part of

of the Vosges, constitute the summits of the Ballon d'Alsace and the Ballon de Comté, range from E. 10° or 15° S. to W. 10° or 15° N., and have thrown up the anthracitic rocks in this direction. The coal-measures of Ronchamps are deposited at the foot of these mountains on the edges of the upheaved beds. The Ballon d'Alsace rises 2586 English feet above the town of Giromagny, built on a level with the coal-measures; and the Ballon de Gebweiler, situated more to the N.E., rises 3067 English feet above the same point. Among those inequalities on the surface of the globe, the date of which we can with probability refer to so remote an epoch, we cannot cite any more considerable.

The transition beds of Britanny and of the Bocage of Normandy, on which the coal-measures of Littry and Plessis are deposited, run in a direction comprised within the above-mentioned limits, as is also the case with the transition beds of Ireland, so ably described by Mr. Weaver. The South of Ireland is a hilly and diversified region, composed of ridges having generally an east and west direction, and attaining their greatest elevation in the mountains of Kerry, where Gurrane Tval, one of Magillycuddy's Reeks, near Killarney, rises 3410 feet above the sea. The transition rocks of the same region have a general direction from east to west, and dip to the north and south with vertical beds in the axes of the ridges. The strata, as they diminish in inclination, on each side form a succession of troughs, the beds dipping rapidly to the north or south, and bending to horizontality between the ridges.

These rocks decline gradually towards the north, and finally pass beneath the unconformable deposits of the old red sandstone and carboniferous limestone of the midland counties; a discordance rendered particularly striking by the horizontal position of the carboniferous limestone of some districts.

In Devonshire and Somersetshire the grauwacké and slates, sometimes containing small seams of carbonaceous matter, also present a nearly east and west direction, and are seen clearly to have been upheaved previous to the deposition of the Exeter red conglomerate or todte liegende, because the latter covers the edges of the former, as may be seen in many situations.

The grauwacké chain of Magdeburg has also a direction comprised within the above-noticed limits; and according to the observations of Professor Sedgwick and Mr. Murchison, it contains the abundant impressions of true coal plants. This same direction is again observed in the older rocks of the Hartz, where we are certain that the dislocations were in part effected

effected prior to the deposition of the secondary beds which extend at the foot of the mountains; and particularly before the formation of the coal-measures of Ilefeld.

This system, joined to that previously noticed, and perhaps also to others which have not yet been studied, has produced an undulated surface and a dislocated structure in the ancient land (*ur und uebergangsgebirge*), in the inequalities of which the first beds of that mass of rocks was deposited which Werner named *flœtz gebirge*, and the English and French geologists secondary deposits, deposits of which the carboniferous series (old red sandstone, mountain limestone, and coal-measures) constitutes the lowest part.

III. *System of the North of England.*—From the latitude of Derby to the frontiers of Scotland, the surface of England is divided by a mountainous axis, which, taken as a whole, runs nearly from south to north, stretching a little towards the N.N.W. In that chain which, being wholly formed of beds of the carboniferous series, is called the great carboniferous chain of the North of England, the forces of elevation appear on the whole to have acted (though not without considerable deviations) on a line bearing nearly north and south (inclining but a few degrees to the N.N.W. and S.S.E.). Hence great faults have originated, by one of which its western limit is tracked through the Peak of Derbyshire. This is prolonged through an anticlinal line into the high western moors of Yorkshire, and there the western escarpment of the chain is accompanied by enormous breaks from the heart of Craven to the foot of Stainmoor. Another enormous break, passing under the escarpment of the Cross-fell range, meets the prolonged line of the Craven fault at an obtuse angle near the foot of Stainmoor. By this last fault the insulated position of the lake mountains is at once explained.

In Professor Sedgwick's memoir, whence the above is derived, we find direct proofs that all the fractures above mentioned took place immediately before the formation of the conglomerates of the new red sandstone (*rothe todte liegende*), and he affords the strongest reasons for believing that they were produced by an action both violent and of short duration; for we pass at once from the inclined and disrupted masses to the horizontal conglomerates now resting upon them; and there is no trace of any effect that indicates a slow progress from one system of things to the other. Lastly, Professor Sedgwick, speculating on the origin of the phænomena described, points to the different crystalline rocks which appear near the carboniferous chain (toadstone of Derbyshire, and whinstone of Cumberland).

The elevation of the chain of the North of England has very probably not been an isolated phænomenon. If we glance at the geological map of England by Mr. Greenough, and that which accompanies the memoir of Dr. Buckland and Mr. Conybeare on the environs of Bristol, we are naturally led to remark that the problematical rocks which pierce and dislocate the coal deposits of Shrewsbury and Colebrooke Dale, and those which constitute the Malvern Hills, appear connected with a series of fractures which run nearly north and south, being prolonged across the recent transition beds and the carboniferous rocks to the environs of Bristol.

The coast, with a north and south direction, which bounds the western part of the department of La Manche, may probably also be due to a fracture of the same class as those of the great carboniferous chain of the North of England.

IV. *System of the Pays Bas and of South Wales.*—From the environs of Aix-la-Chapelle to the small isles of St. Bride's Bay, Pembrokeshire, over a length of about four hundred English miles, the different portions of the carboniferous series, wherever they are not concealed from observation by more recent formations, are seen in a greater or less state of complete dislocation. There are situations, as at Liége, Mons, Valenciennes, the Boulognais, and the Mendip Hills, where they have suffered very considerable contortions and dislocations. Throughout a large portion of this extent, these beds, which in no part rise to great heights, are covered by more recent deposits, resting horizontally on their edges. The vast sheet of recent deposits which covers the carboniferous series between the environs of Boulogne and those of Bristol, might even throw doubt on the mutual connection of the dislocations in the Pays Bas and the coasts of the Bristol Channel: it is nevertheless certain that the dislocations in both situations possess common characters; such as not widely differing from an east and west direction, without however preserving the same line of bearing for great distances, and only producing small protuberances on the surface of the land, notwithstanding the contortions of the beds in the interior.

In the environs of Liége and Aix-la-Chapelle, the direction of the carboniferous beds becomes nearly parallel to that of the argillaceous slates and grauwacké of the Eiffel and the Hundsruck; but it is probable that this arises from the fractures of the carboniferous series having been inflected in such a manner as to follow the ancient dislocations of the preexisting rocks; for it would be difficult not to admit, from the facts previously noticed, that the elevation of the slate and grauwacké of the Eiffel and the Hundsruck, following a direction nearly

nearly N.E. and S.W., was not referrible, like that of the analogous rocks in Westmoreland, to a much more remote epoch.

The dislocation of the coal-measures of Saarbruck is also probably referrible to the same epoch as that of Glamorganshire and the Pays Bas, as it offers nearly the same direction and characters.

In the environs of Bristol the magnesian conglomerate horizontally covers the edges of the dislocated carboniferous beds, and the *grès de Vosges* is seen at Saarbruck in the same position. The elevation of the beds now under consideration ought therefore to be anterior to the deposition of the magnesian conglomerate of Bristol and of the *grès de Vosges;* but as the todte-liegendes (*grès rouge*), properly so called, does not on any point rest on the carboniferous beds elevated in the direction in question, we may be permitted to presume that their elevation took place after the deposit of the todte-liegendes.

V. *System of the Rhine.*—The Vosges and the Swartzwald form two groups of mountains, to a certain extent symmetrical, terminating one opposite the other in two long cliffs, the general directions of which are parallel to each other, and to the course of the Rhine which flows between them from Bâle to Mayence. These two cliffs, between which extends the great valley of Alsace, are the most clearly defined characters of that assemblage of mountains which M. von Buch has grouped together under the name of the system of the Rhine. They are partly formed by beds of the *grès de Vosges*, and appear due to great fractures or faults, with a direction nearly S. 15° W., and N. 15° E., which have broken them after their deposition. The epoch of this disturbance has necessarily preceded that of the deposition of all those beds which extend from one cliff to the other, forming the slightly undulating base of the basin of Alsace, and among which occur the red or variegated sandstone (*grès bigarré*), the muschelkalk, and the variegated marls (*marnes irisées*). The last three formations have extended round the mountains constituting the system of the Rhine, and mark out the winding of the coasts, bathed by the sea during that period of tranquillity which succeeded those commotions, the effects of which have been so well preserved.

VI. *System of the South-west coasts of Britanny, of La Vendée, of Morvan, of the Böhmerwaldgebirge, and of the Thuringerwald.*—The oolitic series, comprising the lias and its inferior sandstone, has been deposited in an assemblage of seas and gulfs which marks out the windings of the various systems of mountains above noticed, and at the same time those of a peculiar

 system,

system, distinguished by the N.W. and S.E. direction of the greater part of its ridges and valleys, and by the beds of the red or variegated sandstone (*grès bigarré*), the muschelkalk, and the variegated marls (*marnes irisées*) being thrown out of their original position, as well as all the more ancient rocks. In the centre of France, near Avallon and Autun, the granitic and porphyritic protuberances of Morvan stretch from N.W. to S.E., disturb the coal-measures, and raise a peculiar *arkose*, contemporaneous with the variegated marls, to their summits; whilst the lias and another *arkose*, which forms its lowest part, extend horizontally to the feet of the same protuberances and form the plains which surround them. The same direction, and in part the same geological circumstances, are observable in the hills, partly granitic, of the S.W. coast of Britanny and La Vendée. These circumstances also appear in that part of the Böhmerwaldgebirge which separates Bavaria from Bohemia, in the Thuringerwald, and in the lines of disturbance in the muschelkalk and the variegated marls (*keuper*) which according to the excellent map of M. Hoffmann run in the same manner from S.E. to N.W. across the nearly flat countries situated between the Hartz and the Taunus. It therefore appears that the elevation of the different parallel chains above mentioned, is referrible to that revolution on the surface of the globe to which the sudden difference observable between the variegated marls and the lias is due.

VII. *System of the Pilas, the Côte d'Or, and of the Erzgebirge.*—Professor Sedgwick has summed up, in his last Address to the Geological Society of London, our knowledge respecting this system. It includes (in Eastern France) the higher elevations of the Côte d'Or and Mont Pilas, the Cevennes, and a portion of the Jura chain. It may be traced towards the valley of the Rhine, where it is suddenly cut off; but it reappears in the chain of the Erzgebirge, between Bohemia and Saxony. It never rises into mountains of the first order, but is marked throughout (as may be seen on a good physical map) by many longitudinal ridges and furrows, ranging nearly parallel to each other in a direction about north-east and south-west. So far the statement is only an enumeration of certain connected facts in physical geography. But it is followed by a coordinate series of geological phænomena.

A number of formations, including in the ascending order the whole oolitic series, enter here and there into the composition of the geographical system above described; and, without exception, wherever they appear all are in turn elevated, broken, or contorted; yet in their lines of range they preserve a parallelism to the general direction of the ridges. On the

the contrary, wherever rocks of an age not older than that of the green-sand or chalk, appear in the vicinity of any portion of this system, they are either found at a dead level and expanded from the neighbouring mountains into horizontal planes, like the sea at the base of a lofty cliff; or if, since their first deposit, they have undergone any great movement, it is shown to have no relation to the bearing of the older ridges, and to have been produced at a later period.

From all these combined facts follow three important consequences. 1st, That the whole system of parallel ridges, from one end to the other, was elevated at the same period of time, after the development of the oolitic series, and before the deposition of the green-sand and chalk. 2ndly, That the action of elevation was violent and of short continuance, for the inclined strata are shattered and contorted; and between them and the horizontal strata there is no intermediate gradation of deposits. 3rdly, That the period of elevation was followed by an immediate change in many of the forms of organic life.

VIII. *System of Mont Viso.*—The French Alps and the S.W. extremity of the Jura, from the environs of Antibes and Nice to those of Pont d'Ain and Lons-le-Saulnier, present a series of crests and dislocations with a direction towards the N.N.W., in which the older beds of the Wealden formation, the green-sand, and the chalk, are upheaved as well as those of the oolitic series. The pyramid of primitive rocks of Mont Viso is traversed by enormous faults, which from their direction evidently belong to this system of fractures. The eastern crests of the Devolny, north from Gap, are composed of the most ancient beds of the system of green-sand and chalk, thrown up in the direction in question, and elevated more than 4700 English feet above the level of the sea. At the feet of these enormous escarpments, are horizontally deposited, near the Col de Bayud, and at more than 2000 feet lower down, those upper beds of the cretaceous system which are distinguished from the rest by the presence of *Nummulites*, *Cerithia*, *Ampullariæ*, and other shells, the genera of which were long considered as not extending deeper in the series than the tertiary rocks. Thus it was between the two portions of that which is commonly termed the series of the Wealden formation, green-sand, and chalk, that the beds of the Mont Viso system have been upraised.

IX. *Pyreneo-Apennine System.* — Professor Sedgwick presented a summary of this system, in his last Address to the Geological Society of London, and I must not omit to mention that important parts of the whole evidence were added by Professor Sedgwick himself and Mr. Murchison, during their

their last travels on the Continent. This system includes the whole chain of the Pyrenees, the northern and some other ridges of the Apennines, the calcareous chains to the N.E. of the Adriatic, those of the Morea, nearly the whole Carpathian chain, and a great series of inequalities continued from that chain through the N.E. escarpment of the Hartz mountains to the plains of Northern Germany. Through the whole of these vast regions the principal inequalities range nearly parallel to each other, and have a mean bearing about west-north-west and east-south-east. So far again the statement is purely geographical, and its truth is seen at once in glancing over any good physical map of Europe; and will be still more clearly comprehended, by comparing some of the principal ranges of colour on Von Buch's great geological map with the bearing of the Pyrenees. But it is followed by a series of co-extensive geological phænomena.

Through all parts of this great system, formations of the age of the green-sand and chalk have had an enormous developments, and without exception, their strata are ruptured and contorted, and often lifted up to the very pinnacles of the mountains. But on the contrary, wherever any tertiary formations approach the confines of this system, they are stated to be either in a position almost as horizontal as the surface of the waters in which they were deposited; or if they have been moved at all, it is by forces uninfluenced by the parallels of the older chains. And the same three conclusions, with a mere difference of dates, follow here as in the former case. All the great parallel ridges and chains of this second system must have been suddenly and violently elevated, and at a period of time between the deposition of the chalk and the commencement of the tertiary groups; and the corresponding change in organic types is, in this instance, still more striking than in the former.

X. *System of the Islands of Corsica and Sardinia.* — The beds named tertiary are far from constituting a continuous whole. Many interruptions are observable in them, each of which may have corresponded with an elevation of mountains effected in countries more or less near our own.

An attentive examination of the nature and geographical disposition of the tertiary rocks in the north and south of France, has led me to divide them into two series: one, which is composed of the plastic clay, the *calcaire grossier*, and the whole gypseous formation, including the upper marine marls, scarcely passes to the S. or S.W. of the environs of Paris; whilst the other, represented in the North by the *grès de Fontainebleau*, the upper freshwater formation, and the *fahluns*, comprises,

comprises, with few exceptions, nearly the whole tertiary deposits of the South of France and Switzerland, and especially the lignite deposits, such as those of Fureau (*Bouches du Rhone*), and Kœpfnach (Switzerland). The *grès de Fontainebleau*, resting on the marls of the gypseous formation, is the lowest portion of this series, in the same manner that the lias sandstone, resting on the variegated marls (*marnes irisées*), is the lowest portion of the oolitic series. The former is to the tertiary *arkose* of Auvergne, what the latter is to the Jurassic *arkose* of Avallon. The two tertiary series are not less distinguished by the remains of the large animals which they contain, than by their mode of occurrence. Certain species of *Anoplotherium* and *Palæotherium* discovered at Montmartre, characterize the former, whilst other species of *Palæotherium* and nearly all the species of the genus *Lophiodon*, the whole genus *Anthracotherium*, and the more ancient species of the genera *Mastodon*, *Rhinoceros*, *Hippopotamus*, *Castor*, &c., characterize the latter.

The line of demarcation existing between the first and second of these tertiary series would appear to correspond with the elevation of the system of mountains under consideration, the predominant direction of which is from north to south. The beds of the second series are, in fact, those which alone mark out the boundaries of the mountains.

Among the dislocations with a north and south direction, we find the chains which border the high valleys of the Loire and Allier, in a similar line of bearing to which are the volcanic masses of the Dome mountains, and at the bottoms of which the fresh-water rocks of Limagne, of Auvergne, and of the high valley of the Loire have been accumulated. The valley of the Rhone which, quitting Lyon, also runs in a north and south direction, is in like manner filled up to a certain level by a tertiary deposit, the inferior beds of which, analogous to those of Auvergne, are also of fresh-water origin, while the upper beds are marine, and in a great measure correspond with the *fahluns* of Touraine.

The same direction is observable in the islands of Corsica and Sardinia, in many valleys and small chains of the Apennines and of Ystria, in the disposition of many volcanic masses and metalliferous sites of Hungary, and the chain which, commencing in the middle of Servia with the Caponi, is prolonged, parallel to the meridian between Macedonia and Thessaly on the one side, and Albania on the other, bordering the valleys of the Drino and the Arta on the east.

It is worthy of remark, that the directions of the system of the Pilas and the Côte d'Or, of the system of the Pyrenees, and

and that of the islands of Corsica and Sardinia, are respectively nearly parallel to those of the system of Westmoreland and the Hundsruck, of the system of the Ballons and the hills of the Bocage, and of the system of the North of England. The corresponding directions only differ in a few degrees, and the two series have succeeded each other in the same order; leading to the supposition that there has been a *kind of periodical recurrence* of the same, or nearly the same, directions of elevation.

XI. *System of the Western Alps.*—The opinions in accordance with which M. Jurine named the granitic rock constituting Mont Blanc *Protogine*, can no longer be sustained. The tertiary beds which have been deposited horizontally in that part of the valley of the Rhone which runs N. and S. are constantly contorted and thrown up as they approach the Alps. A similar observation has been made in the valley of the Danube by Professor Sedgwick and Mr. Murchison, who found the cretaceous and tertiary beds to extend horizontally to the foot of the Bohemian mountains, and to be thrown up on entering the Austrian Alps. Messrs. Lyell and Murchison have made analogous observations on the tertiary rocks of Lombardy. Professor Buckland and M. Brongniart have pointed out the tertiary aspect of the fossil shells discovered at the Diablerets, at more than 8000 feet above the level of the sea; shells the relative age of which certainly does not go back beyond the last portion of the cretaceous epoch.

Although we are generally accustomed to consider the union of those mountains bearing the single name of *the Alps* as constituting an undivided whole, we can easily recognise that this vast assemblage is due to the crossing of several systems, independent of each other, and distinct both in age and direction. We should therefore not feel surprise that their structure is more confused than that of a chain thrown up by a single effort, such as the Pyrenees. Throughout nearly their whole extent, and especially on their eastern side, we still perceive traces of numerous small chains of mountains with the same direction as the Pyrenees, and elevated in like manner prior to the deposition of the tertiary rocks. The system of Mont Viso is strongly marked in the French Alps. These traces of comparatively ancient dislocations are, however, often marked by disturbances of a more recent date.

The highest and most complicated portions of the Alps, those near the Mont Blanc, Mont Rose, and the Finsteraarhorn, are principally due to the crossing of two recent systems which meet at an angle of from 45° to 50°, and which are distinguishable from the system of Mont Viso and the Pyreneo-Apennine system, as well by their age as their directions. In consequence

consequence of the crossing of these two systems of furrows and ridges, the French Alps form an elbow near the Mont Blanc, and after having followed a direction from E. $\frac{1}{4}$° N.E. to W. $\frac{1}{4}$° S.W. from Austria to the Valais, they suddenly turn to fall into a line from N.N.E. to S.S.W. If there was only a simple curve in a single chain of mountains which merely formed an arch, we should find the direction of the beds to bend and pass from the direction of one of the systems to that of the other. We however observe, that the direction of the beds and crests distinctly belong either to one or to the other, and that the two systems penetrate each other, as we should conceive they must do if they are the productions of two entirely distinct phænomena.

In the Western Alps, that is to say, to the westward of the St. Gothard, and particularly in the mountains of Savoy and Dauphiny, the greater part of the dislocations are referrible to two systems of ridges, the mean direction of which is N.N.E. and S.S.W., or more exactly N. 26° E., and S. 26° W. The constant direction of the beds in these mountains has long since been remarked by De Saussure, and more recently by M. Brochant; and they with reason concluded, that in all those parts where this direction predominated, the beds were thrown up by a single operation of nature.

It is easy to determine the geological date of this event; for we have only to examine what are the formations which have been disturbed, and what the deposits which extend horizontally on the edges of the dislocated and more ancient strata.

In the interior of that system of ridges of which the Western Alps are principally composed, we do not find beds more recent than the chalk, because these ridges have been formed on a surface previously made mountainous, at the epoch of the systems of Mont Viso and the Pyrenees. But on the skirts, as also at the two extremities of the space occupied by the ridges to which the character of the Western Alps is due, we find that the dislocations which have produced the ridges are carried into the most recent tertiary deposits, as well as into the secondary rocks which support them: whence it follows, that the elevation of the beds in the system of the Western Alps took place after the deposit of those recent tertiary beds, named shelly molasse (*mollasse coquillière*), beds contemporaneous with the *fahluns* of Touraine.

XII. *System of the principal Chain of the Alps* (*from the Valais to Austria*), *comprising also the Chains of the Ventoux, the Liberon, and the St. Baume* (*Provence*).—The valleys of the Isère, the Rhone, the Saone, and the Durance, present two very distinct detrital and transported formations, between

which there is a want of continuity, and a sudden variation of character, constituting a new interruption in the series of sedimentary deposits.

The waters which have transported the materials of the first of these formations would appear to have been received into lakes of fresh water which covered, in one direction, the N.W. portion of the department of the Isère, La Bresse, and perhaps, Alsace, and even the environs of the lake of Constance; and in the other, the portion of the department of the Basses Alpes between Digne, Manosque, and Barjols: whilst the materials of the second formation appear to have been violently carried by temporary currents which have discharged themselves into the Mediterranean. These latter currents are generally known as *diluvial currents*, though they offer nothing in common with the Deluge of history, and though their passage took place before the human race appeared on our continent, where they destroyed animals of species now extinct. Discussions will still perhaps be carried on respecting their origin, which may have merely been the result of the melting of the snows, instantaneously effected when the principal chain of the Alps was elevated; but it seems generally admitted that their passage immediately followed the last dislocation of the Alpine strata.

If we cast a general glance on the Alps and neighbouring countries, we may observe that the crests of the St. Baume, the Lebaron, the Ventoux, and the Montagne de Poet, in the South of France, the principal chain of the Alps from the Valais to Austria, and the less elevated crest, comprising the Pilate, &c. in Switzerland, are so many different chains, which, notwithstanding their inequality, are comparable with each other both as respects their parallelism and their common analogies to the system of the Western Alps. This parallelism and these analogies would alone afford us powerful reasons for believing that the whole of these mountain-chains were formed at the same time, and are only different parts of a single system of fractures produced at the same moment. We can at furthest conceive the idea of dividing them into two groups, —that of Provence, and that of the Alps; but we are prevented from doing this by the analogous relations observable among the different fractures, and by a general movement which we may consider the surface of a part of France to have suffered when it contracted a double slope; ascending in one direction from Dijon and Bourges towards Le Forez and Auvergne, and on the other from the shores of the Mediterranean towards the same countries. These opposed slopes present at their junction a kind of crest, situated precisely in the line of elevation

tion of the principal chain of the Alps. This line, which may be observed to run in a more or less marked manner from the confines of Hungary to Auvergne, appears to be connected with the principal anomalies unveiled in the interior structure of our continent by geodesical measurements and observations with the pendulum. We may even suppose that the formation of this line gave, as it were, the signal for the appearance of the craters of elevation of the Cantal and Mont d'Or, round which the volcanic cones of Auvergne have been subsequently thrown up.

The two opposite slopes, above mentioned, were not produced until after the existence of those lakes in which the older transported substances were accumulated; for it can be ascertained that the bottom of the lake which covered La Bresse and the N.W. portion of the department of the Isère has suffered a considerable elevation from the north towards the south, and that the bottom of the lake which extended between Digne, Manosque, and Barjols, has been elevated to a great degree from the south towards the north.

The ancient deposits of transported substances, forming horizontal beds at the bottom of the latter of these lakes, on the edges of tertiary deposits, previously dislocated when the Western Alps were thrown up, are in their turn dislocated near Mezel (Basses Alpes) in the direction of the small chains which ridge Provence, such as the Ventoux and Lebaron, parallel to the principal chain of the Alps.

To determine the date of this last order of dislocations it will be sufficient to remark, that the diluvian deposit is in no part affected; that it covers the edges of the dislocated beds with no other slope than that which the current impressed on them at their origin; and that thus the elevation of the beds in question necessarily took place between the older deposit of transported substances and the passage of the diluvian currents.

If we attentively consider, on a terrestrial globe of sufficient size and good execution, the most prominent and the most recent systems of mountains which ridge Europe, we may remark that each of them forms a part of a vast system of parallel chains, which extends far beyond the countries geologically known to us. But as in all the parts of each of these systems situated in well examined portions of Europe, it has been more and more observed that parallel chains are in general contemporaneous, there is no reason to suppose that this law should suddenly cease, if its verification should be pushed still further. It is therefore natural to consider, until direct observations may show the contrary, that each of

these vast systems, of which the European systems are respectively portions, originates in a single epoch of dislocation. From this view I am led to suppose, for example, that the principal chain of the Alps is contemporaneous with a vast assemblage of mountain-chains which spread round the Mediterranean, and being prolonged across the continent of Asia, run parallel to a great circle which should pass through the middle of Morocco and the north of the Birman empire, and appear at the same time connected with each other by parallelism and by the similarity of their relations to the great depressions of surface filled by the sea, or but slighty raised above its level. Besides the principal chain of the Alps, and the small chains of Provence, this system comprises, in Europe, the Sierra Morena, and a large portion of the Spanish chains, on the one hand, and the Balkan on the other: in Africa, it includes the Atlas: in Asia, the central trachytic chain of the Caucasus, crowned by the peak of Elbrouz, more elevated than the Mont Blanc, as also the long series of mountains which under the names of Paropamissus, Indou-Kosh, and Himalaya, bound the plains of Persia and Bengal, and contain the most elevated mountains on the surface of the earth.

I am also led to suppose that the system of the Western Alps constitutes a portion of a vast system, comprising the chain of Kiöl in Scandinavia, the chains which in Morocco run from Cape Tres Furcas to Cape Blanc, and the Littoral Cordilera of Brazil.

Finally, I am led to suppose that the chains of the Pyreneo-Apennine system observed in Europe, form a portion of a vast system comprising certain chains in the north of Africa, of Egypt, of Syria, of the Caucasus, the chains which bound Mesopotamia on the north-east, and even the Ghauts of Malabar, and which appears in another direction, across the Atlantic, in the Alleghanies.

The appearance of a new system of mountains which, judging from the result of our observations, has produced such violent effects on countries near them, could only have exercised an influence in distant countries by the agitation caused in the waters of the sea, and by a greater or less change produced in their level,—events which may be compared to the sudden and passing deluge noticed among the traditions of all nations as having occurred at nearly the same epoch. If this historical event was the last which has taken place on the surface of the globe, we are naturally led to inquire which is the mountain-chain referrible to the same date: and perhaps we may be justified in observing that the chain of the Andes, whose volcanic

volcanic vents are still in activity, (or more exactly the long cliff (*falaise*) surmounted or bounded by volcanos which run on a great semicircle of the earth from Chili to the Birman country,) presents the most extensive, the most clearly defined, and as it were the least obliterated feature observable in the present exterior configuration of the globe.

It has been shown, as Professor Sedgwick justly observes, that paroxysms of internal energy, accompanied by the elevation of mountains, and followed by mighty waves desolating whole regions of the earth, were a part of the mechanism of Nature; and what has happened again and again, from the most ancient up to the most modern periods, may have happened once during the few thousand years that man has been living on its surface. We have therefore taken away all anterior incredibility from the fact of a recent deluge.

If the general result of the preceding observations be exact, we may briefly express it by saying, that the independence of sedimentary formations is both a consequence and proof of the independence of mountain-systems having different directions. Many traces of interruptions in the series of sedimentary deposits are, perhaps, so slight in Europe, only because they correspond with mountain-systems which, like that so strongly marked on the shores of Mozambique and Madagascar, have not sent any ramifications into our countries.

But if the number of the surface-revolutions of the globe, and of really distinct mountain-systems be still undetermined, and if the series formed by these successive terms be still imperfectly known, the observations already made nevertheless circumscribe within certain limits that law, which when they shall be all completely known may be manifested in their succession. From the circumstance of the present heights of Mont Blanc and Mont Rosa, dating only from the later surface-revolutions of the globe, it is clear, that whatever definitive place other and higher mountains may occupy in the same series, this series will never take that gradually and regularly decreasing form which should lead to the conclusion, that the limit was attained. Nothing will show that phænomena the last paroxysms of which have been so violent should not be reproduced. However provisional the succession of terms may be which results from the preceding memoir, it is difficult to foresee a modification which should so change its aspect, as to lead to the supposition, that the mineral crust of the globe has lost the property of being successively ridged in various directions. It is difficult to conceive a change which would permit us to assure ourselves that the period of tranquillity

quillity in which we live will not be disturbed in its turn by the appearance of a new system of mountains, the effect of a new dislocation of the land we inhabit, and of which earthquakes teach us the foundations are not immovable.

The independence of successive sedimentary formations is the most important result obtained from the study of the superficial beds of our globe; and one of the principal objects of my researches has been to show, that this great fact is a consequence, and even a proof, of the independence of mountain-systems having different directions.

The fact of a general uniformity in the direction of all beds upheaved at the same epoch, and consequently in the crests formed by these beds, is perhaps as important in the study of mountains, as the independence of successive formations is in the study of superimposed beds. The sudden change of direction in passing from one group to another has permitted the division of European chains into a certain number of distinct systems, which penetrate, and sometimes cross each other without becoming confounded. I have recognised from various examples, of which the number now amounts to twelve, that there is a coincidence between the sudden changes established by the lines of demarcation observable in certain consecutive stages of the sedimentary rocks, and the elevation of the beds of the same number of mountain-systems.

Pursuing the subject as far as my means of observation and induction will permit, it has appeared to me, that the different systems, at least those which are at the same time the most striking and recent, are composed of a certain number of small chains, ranged parallel to the semicircumference of the surface of the globe, and occupying a zone of much greater length than breadth; and of which the length embraces a considerable fraction of one of the great circles of the terrestrial sphere. It may be observed in support of the hypothesis of each of these mountain-systems being the product of a single epoch of dislocation, that it is easier geometrically to conceive the manner in which the solid crust of the globe may be elevated into ridges along a considerable portion of one of its great circles, than that a similar effect may have been produced in a more restricted space.

However well established it may be by facts, the assemblage of which constitutes positive geology, that the surface of the globe has presented a long series of tranquil periods, each separated from that which followed it by a sudden and violent convulsion, in which a portion of the earth's crust was dislocated,—that, in a word, this surface was ridged at intervals in

in different directions; the mind would not rest satisfied if it did not perceive, among those causes now in action, an element fitted from time to time to produce disturbances different from the ordinary march of the phænomena which we now witness.

The idea of *volcanic action* naturally presents itself when we search, in the existing state of things, for a term of comparison with these great phænomena. They nevertheless do not appear susceptible of being referred to volcanic action, unless we define it, with M. Humboldt, as being *the influence exercised by the interior of a planet on its exterior covering during its different stages of refrigeration.*

Volcanos are frequently arranged in lines following fractures parallel to mountain-chains, and which originate in the elevation of such chains; but it does not appear to me that we can thence regard the elevation of the chains themselves as due to the action of *volcanic foci,* taking the words in their ordinary and restricted sense. We can easily conceive how a *volcanic focus* may produce accidents circularly and in the form of rays from a central point, but we cannot conceive how even many united *foci* could produce those ridges which follow a common direction through several degrees.

Volcanic action, such as it is commonly understood, could not therefore be itself the first cause of these great phænomena; but volcanic action appears to be related (and this is a subject which has long occupied M. Cordier, though he has considered it under another point of view) with the high temperature now existing in the earth.

Now the secular refrigeration, that is to say, the slow diffusion of the primitive heat to which the planets owe their spheroidal form, and the generally regular disposition of their beds from the centre to the circumference, in the order of specific gravity,—the secular refrigeration, on the march of which M. Fourier has thrown so much light, does offer an element to which these extraordinary effects may be referred. This element is the relation which a refrigeration so advanced as that of the planetary bodies establishes between the capacity of their solid crusts and the volume of their internal masses. In a given time, the temperature of the interior of the planets is lowered by a much greater quantity than that on their surfaces, of which the refrigeration is now nearly insensible. We are, undoubtedly, ignorant of the physical properties of the matter composing the interior of these bodies; but analogy leads us to consider, that the inequality of cooling above noticed would place their crusts under the necessity of

continually

continually diminishing their capacities, notwithstanding the nearly rigorous constancy of their temperature, in order that they should not cease to embrace their internal masses exactly, the temperature of which diminishes sensibly. They must therefore depart in a slight and progressive manner from the spheroidal figure proper to them, and corresponding to a maximum of capacity; and the gradually increasing tendency to revert to that figure, whether it acts alone, or whether it combines with other internal causes of change which the planets may contain, may, with great probability, completely account for the ridges and protuberances which have been suddenly formed at intervals on the external crust of the earth, and probably also of all the other planets.

XXXIII. *On an undescribed Bird of the Family* Falconidæ. *By* John Blackwall, *Esq. F.L.S. &c.**

DURING the last two years, five specimens of a minute Hawk, no account of which, there is reason to believe, has yet been published, have been brought to Manchester, at different periods, from Brazil. On inspecting this new species, it is evident from several peculiarities in its organization, that it should occupy a situation, in a natural arrangement of birds, intermediate between the Hawks and true Falcons; as it unites in itself certain features characteristic of each of those groups. Its short bill, curved from the base, the upper mandible of which is furnished on each side with a small festoon; the shortness of its wings, notwithstanding the second quill-feather is the longest, and the first has the inner web slightly emarginated near its termination; the moderate length of the tail and legs; the reticulated tarsi, and the acrotarsia feathered from the knee to the middle,—plainly indicate that it must be referred to the genus *Gampsonyx*, established by Mr. Vigors.

Order.	*Raptores.* Illiger.
Family.	*Falconidæ.* Leach.
Subfamily.	*Accipitrina.* Vigors.
Genus.	*Gampsonyx.* Vigors.

G. Holmii. The bill, which is much curved, is black faintly tinged with blue. Plumage on the forehead and cheeks pale orange; that on the top of the head, back, scapulars and upper part of the wings, dark cinereous brown. Greater wing-coverts and feathers of the spurious wings

* Communicated by the Author.

white

white at their extremities. Each quill-feather of the wings has a broad margin of white on its inner web, and the secondaries and tertials are tipped with white. Upper tail-coverts and feathers of the tail deep cinereous; the latter, with the exception of the two middle ones, which are plain, having a border of white on their inner webs. A white collar passes over the back part of the neck, immediately behind which is a narrow parallel band of a chestnut colour. On each side of the breast a black spot is conspicuous. All the inferior parts are white except the thighs, which are of a bright rust colour, and the under coverts of the wings, which exhibit a slight tint of the same hue. Legs and feet yellow. Claws dark horn colour inclining to black. Colour of the eyes not known. Total length 9 inches; wings, from the carpus to the tip of the second quill-feather, $5\frac{9}{10}$; upper mandible, from the point to the gape, in a straight line, $\frac{7}{10}$; under mandible, $\frac{6}{10}$; tarsi, 1.

The specimen from which the foregoing description was taken occupies a place in the Manchester Museum.

That *G. Holmii* bears a striking resemblance to the *G. Swainsonii* of Mr. Vigors cannot be denied; the white collar and chestnut-coloured band on the neck of the former, and the pure white plumage of its abdomen, constituting the most obvious points of difference between the two species.

To Edward Holme, M.D., the learned and accomplished President of the Natural History Society of Manchester, (who has uniformly promoted my zoological investigations by every assistance which his extensive knowledge and valuable library could supply,) this bird is respectfully dedicated.

XXXIV. *On* Monticellite, *a new Species of Mineral; on the Characters of* Zoizite; *and on* Cupreous Sulphate of Lead. *By* H. J. Brooke, *Esq. F.R.S. L.S. & G.S.**

Monticellite.

I OBTAINED a year or two since from Mr. G. B. Sowerby, a specimen said to have come from Vesuvius, containing some imbedded crystals of a substance which I believe has not been before noticed, and of which I am not aware of having seen any other specimen. The matrix is crystalline carbonate of lime; and besides the mineral I am about to describe, it contains particles of black mica, and some minute crystals of pyroxene. On the supposition of its being an undescribed mineral, and from Vesuvius, I have named it after Mr. Monti-

* Communicated by the Author.

celli, who has published a work in illustration of the minerals found in the neighbourhood of that mountain. The general aspect of the crystals is that of quartz, and might by a cursory observer be mistaken for it. The colour generally is yellowish, but there are some crystals nearly colourless and nearly transparent; and on placing a portion of the specimen in dilute muriatic acid to dissolve the carbonate of lime, I found that the surfaces of the yellowish crystals became dull, and were covered with a yellowish powder, leaving the crystals less coloured than they were at first.

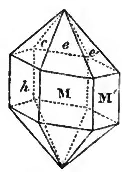
The primary form is a *right rhombic prism* of about 132° 54′, a terminal edge being to a lateral edge as 1 to 1·046 very nearly.

I have not observed any cleavage planes on the fractured surfaces, and the crystals are too small to allow of much other examination in this respect. The hardness is between that of apatite and felspar. There is no crystal sufficiently free from the matrix to allow of the specific gravity being ascertained; nor are the surfaces of the crystals sufficiently perfect to afford very accurate measurements. The following therefore may admit of some slight correction:

Planes .... e, c, h.

Symbols .... $\overset{1}{B}$, $\overset{1}{E}$, $\overset{1}{G}$.

$M,M' = 132°\ 54'$
$M,e = 145\ 00$
$e,e' = 141\ 48$
$h,c = 138\ 46$
$M,h = 113\ 33$



Zoizite.

This mineral has been confounded with *Epidote* by Haüy, probably from the occurrence of crystals of that substance in the *Zoizite* of Hoff; and in this mistake he has been followed by most other writers on the subject.

The late W. Phillips says, "it cleaves parallel to the planes of a right rhombic prism of about 60° and 120°."

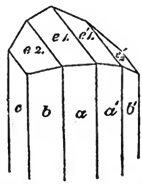
Mr. Haidinger, in his Treatise on Mineralogy, says that Epidote and Zoizite are easily distinguished by their *colours.* And in reference to the angle which I had given as that of Zoizite, differing from the angle of Epidote, he says; "this would render it necessary to consider Zoizite as a particular species." Hence it is clear that Mr. Haidinger could not have examined this mineral; for if he had, he must immediately have perceived its difference from Epidote.

I have lately obtained a small crystal of Zoizite with terminal planes,

planes, from which it is evident that the primary form is an oblique rhombic prism, and from the angles given below it approaches very nearly, if it be not the same as that of euclase. It has also a bright cleavage through the oblique diagonal, similar to that of euclase, and no very distinct cleavage in any other direction.

The annexed figure exhibits the form of the crystal I have alluded to, the terminal planes of which are, however, too imperfect to afford accurate measurements.

$e1,a = 123° \; 30'$ (about)*
$a,c = 107 \; 20$ ... $P,c9 = 107° \; 20'$
$b,c = 121 \; 45$ ... $P,c2 = 121 \; 30$
$a,a' = 145 \; 20$
$b,b' = 116 \; 30$



The perfect identity of the forms of Zoizite and Euclase depends obviously on the relative *dimensions* of the primary forms, as well as upon the angles of the prisms; and as those dimensions can be deduced only from accurate measurements of the terminal planes, it is to be hoped that those who possess better crystals will supply the deficient angles, and complete the description of the form.

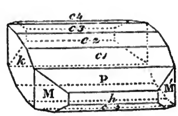
Cupreous Sulphate of Lead.

A specimen I have lately obtained of this substance has enabled me to give the annexed figure and measurements of the crystals. The primary form is an *oblique rhombic prism*, a terminal edge of which is to a lateral edge as 19 to 8 very nearly, the plane angles of the terminal plane at the extremities of the oblique diagonal being 59° 12'.

Planes ...... $c1$, $c2$, $c3$, $c4$, $c5$, h.

Symbols .... $\overset{8}{A}$, $\overset{\frac{3}{4}}{A}$, $\overset{\frac{6}{5}}{A}$, $\overset{1}{A}$, $\overset{\frac{1}{2}}{A}$, $\overset{1}{H}$.

$P,M = 96°25'$
$P,h = 102 \; 45$
$P,c1 = 176 \; 35$
$P,c2 = 161 \; 30$
$P,c3 = 156 \; 10$
$P,c4 = 151 \; 40$
$P,c5 = 129 \; 40$
$M,M' = 61$



* Corresponding planes of euclase, as measured by W. P. (See his figure.)

 Hemitrope

Hemitrope crystals occasionally present themselves, the plane of revolution being parallel to the plane *h*, which truncates the edge of the prism. The angle at which the two planes P then intersect each other, is 154° 30′.

XXXV. *On a new Register-Pyrometer, for Measuring the Expansions of Solids, and determining the higher Degrees of Temperature upon the common Thermometric Scale.* By J. Frederic Daniell, *Esq. F.R.S.*

[Continued from p. 199.]

I SHALL now proceed to show the degree of confidence which may be placed in this new pyrometer, by comparing the result of its indications with those of the best experiments upon the expansion of metals. Those of MM. Dulong and Petit* are well adapted to this purpose. These able philosophers, in their celebrated prize Memoir on the Measure of Temperatures, and on the Laws of the Communication of Heat, have given, from experiment, the expansion of rods of platinum and iron at different intervals between the freezing point of water and the boiling of mercury. Their mode of experimenting was unexceptionable; but it is to be regretted that they have not corrected their final results for an error of calculation which has been pointed out by Mr. Crichton† which is by no means unimportant to the reasoning which they have founded upon them. The error, however, affecting the amount of expansion in volume, is reduced to one-third in the linear expansion, which is the subject of the present investigation, and may therefore be disregarded.

The following Table of the expansion of iron and platinum is extracted from their work.

Table II.

Temperature deduced from the Dilatation of Air.	Mean absolute Dilatation of Iron for 180 degrees.	Mean absolute Dilatation of Platinum for 180 degrees.
From 32° to 212°	$\frac{1}{28200}$	$\frac{1}{37700}$
From 392° to 572°	$\frac{1}{22700}$	$\frac{1}{36300}$

Whence we deduce the linear expansion of platinum for 180° Fahrenheit, from 32° to 212° ·00088420: and for 180°, from

* *Ann. de Chimie et Physique*, vii. 113.

† Annals of Philosophy, New Series, vol. vii. p. 241.

from 392° to 572° ·00091827: and of iron, from 32° to 212° ·00118203: from 392° to 572° ·00146842, showing an increasing dilatation in each when referred to an air-thermometer.

The bars of the different metals used in the following experiments were all exactly 6·5 inches in length.

Exp. 1. A square bar of platinum $\frac{2}{10}$ths of an inch thick, was carefully arranged in the black-lead register, which was placed in the apparatus represented, upon a diminished scale, at fig. 3. (Plate II.) *a* is an iron tube about two inches diameter, and closed at the botom: *b* is a black-lead tube closed at the top, and fitted to the mouth of the former by grinding: *c* is a smaller black-lead tube projecting from the side of the latter near its upper end, and likewise fitted to its place by grinding. The whole forms a kind of alembic, which may be readily put together, and in which mercury may be easily boiled on a common fire, and the vapours collected without loss or annoyance to the operator. The register was fixed in its place by a wire, so that when mercury was poured into the iron bottle it was prevented from floating. The mercury in this experiment rose a little above half the length of the register. The whole apparatus was then placed upon a fire, and in ten minutes the mercury began to boil: in ten minutes more it freely distilled over; and in ten minutes further the apparatus was removed, the register taken out and allowed to cool. The arc measured upon the scale was in this instance 1° 17′.

The experiment was repeated, merely having the head of the alembic off, and suffering the mercury to boil freely in the iron bottle for a quarter of an hour. The arc measured was 1° 23′.

The register was next allowed to float upon the mercury, so that when the head of the alembic was adjusted and the mercury made to boil, it was not immersed in the metal, but surrounded by its vapour: the reading was 1° 16′. A repetition of this arrangement gave 1° 23′.

In another repetition of the experiment, the time was extended to twenty minutes from the first boiling of the mercury; the reading of the scale was 1° 20′.

Again; the time was reduced to ten minutes, and the measurement was 1° 23′.

In the various repetitions of this experiment the mercury freely distilled over, and the temperature was such, that every part of the black-lead tubes, in which the vapour circulated, would just scorch, but not blacken, a piece of writing paper held against them.

The following Table collects these results into one view, and exhibits the expansion denoted by each reading, and the mean result.

TABLE

TABLE III.

1° 17′	=	·01119
1 23	=	·01206
1 16	=	·01105
1 23	=	·01206
1 20	=	·01163
1 23	=	·01206—Mean 1° 20′ = ·01163.

The temperature of the atmosphere was about 64° during these observations.

Exp. 2. A bar of soft iron, of the same dimensions as that of platinum, was substituted for the latter in the register. The experiment was repeated five times; twice with the register immersed in the mercury, and three times exposed only to the vapour. The time of exposure varied from twenty minutes to ten, from the first moment when the metal began to boil.

The following Table exhibits the several readings and the appropriate expansions.

TABLE IV.

2° 13′	=	·01933
2 33	=	·02224
2 10	=	·01890
2 23	=	·02079
2 20	=	·02036—Mean 2° 20′ = ·02036.

The greatest variation from the mean was therefore only $\frac{6}{10,000}$dths of an inch in the platinum experiment, and $\frac{13}{10,000}$dths in the iron.

We shall now compare these results with the preceding determinations of MM. Dulong and Petit.

The Expansion of Platinum.

	Length of Bar.	
From 32° to 212° = ·00088420 × 6·5	....	=·005747300
From 392° to 572° = ·00091827 × 6·5	....	=·005968755
		·011716055
From 212° to 392° = Mean of the above	....	=·005858027
Total expansion from 32° to 572°	....	=·017574082
Add for the expansion from 572° to 660°, the temperature of boiling mercury, calculated at the highest rate:—		
180° : ·005968755 : : 88° : ·002918058	....	=·002918058
		·020492140
Deduct expansion for 32°, the experiment with the pyrometer having been made at 64°	...	=·001021742
Calculated at the lowest rate:—		
180° : ·005747300 : : 32° : ·001021742		
Real expansion of the bar by Dulong and Petit		=·019470398

If

If from the real expansion thus obtained...............	·01947
we deduct the apparent expansion obtained by the pyrometer...	·01163
the remainder	·00784

will be the expansion of the black-lead.

The Expansion of Iron.

	Length of Bar.	
From 32° to 212° =	·00118203 × 6·5.........	=·007683195
From 392° to 572° =	·00146842 × 6·5.........	=·009544730
		·017227925
From 212° to 392° =	Mean of the above	=·008613962
Total expansion from 32° to 572°...............		=·025841887
Add for the expansion from 572° to 660°, the temperature of boiling mercury, calculated at the highest rate:— 180° : ·009544730 : : 88° : ·004666311		=·004666311
		·030508198
Deduct expansion for 32°, the experiment with the pyrometer having commenced at 64° ...		=·001365901
Calculated at the lowest rate:— 180° : ·007683195 : : 32° : ·001365901		
Real expansion of the bar by Dulong and Petit		·029142297

From the real expansion....................................	·02914
deduct the apparent expansion obtained by the pyrometer...	·02036
The remainder	·00878

is again the expansion of the black-lead as obtained by this series of experiments.

Expansion of 6·5 inches of black-lead.

From 64° to 660° by platinum bar	·00784
by iron bar	·00878
Mean	·00831

either determination differing from the mean by less than $\frac{5}{10\cdot000}$dths of an inch.

This close agreement in results from two metals whose expansions differ so much from each other is highly satisfactory; but the great delicacy of the instrument may be still better appreciated from the following experiment of the expansion of nine different metals from the temperature of 62° (the temperature of the air at the time of observation) to 212°.

Exp. 3.

Exp. 3. Bars of the following metals were successively placed in the register and immersed in hot water, which was gradually heated to the boiling point, and kept boiling for ten minutes in each instance. The following Table exhibits the readings of the scale and the appropriate expansions.

TABLE V.

Platinum.........	0° 19′ =	·00276	from 60° to 212°
Iron (soft)	0 35 =	·00508	———
Copper..........	0 47 =	·00683	———
Tin (grain)......	0 56 =	·00814	———
Zinc...............	1 40 =	·01454	———
Lead..	1 25 =	·01223	———
Brass..............	0 55 =	·00799	———
Gold (fine)	0 36 =	·00552	———
Silver (fine)......	0 56 =	·00814	———

In the subsequent Table, I have given the absolute expansions of the same metals from 32° to 212° from the best authorities; and for the sake of comparison have added from calculation their expansion from 62° to 212°, by reducing the former in the proportion of 180 : 150.

TABLE VI.

	Length of Bar.	From 32° to 212°.	From 62° to 212°.	Authorities.
Platinum	·00088420 × 6·5 =	·005747300 =	·004789416	Dulong & Petit.
Iron......	·00118203 × 6·5 =	·007683195 =	·006402662	Dulong & Petit.
Copper..	·00171821 × 6·5 =	·011168365 =	·009306970	Dulong & Petit.
Tin.......	·00217298 × 6.5 =	·014124370 =	·011770308	Lavoisier & Lapl.
Zinc.....	·00294200 × 6·5 =	·019123000 =	·015935833	Smeaton.
Lead......	·00284836 × 6·5 =	·018514340 =	·015428616	Lavoisier & Lapl.
Brass.....	·00193000 × 6·5 =	·012545000 =	·010454166	Smeaton.
Gold.....	·00146606 × 6·5 =	·009529390 =	·007941158	Lavoisier & Lapl.
Silver....	·00190974 × 6·5 =	·012413310 =	·010344424	Lavoisier & Lapl.

Upon deducting from the amount of these several absolute expansions the apparent expansions in the black-lead register, we shall obtain the expansion of the latter from 62° to 212°, as derived from the several metals. The results are comprised in the following Table.

TABLE VII.

	Expansion of the Metal Bars.		Expansion of Black-lead Register.	Difference from Mean.
Platinum	absolute	·00478		
	apparent	·00276		
		——— ...	= ·00202 ...	— ·00032
Iron	absolute	·00640		
	apparent	·00508		
		——— ...	= ·00132 ...	— ·00102

Copper

TABLE VII. (Continued.)

		Expansion of the Metal Bars.	Expansion of Black-lead Register.	Difference from Mean.
Copper	absolute	·00930		
	apparent	·00683	=·00247	+·00013
Tin	absolute	·01177		
	apparent	·00814	=·00363	+·00129
Zinc	absolute	·01593		
	apparent	·01454	=·00139	−·00095
Lead	absolute	·01542		
	apparent	·01223	=·00319	+·00085
Brass	absolute	·01045		
	apparent	·00799	=·00246	+·00012
Gold	absolute	·00794		
	apparent	·00552	=·00242	+·00008
Silver	absolute	·01034		
	apparent	·00814	=·00220	−·00014
			Mean ·00234	

In five instances out of these nine, the difference of the expansion of the black-lead from the mean does not exceed $\frac{32}{100,000}$dths of an inch, two being in deficiency, and three in excess: and it is worthy of observation that they are the metals whose dilatations have always been considered the most regular, and concerning which there is the least difference of authorities, viz. gold, silver, platinum, copper, and brass. The greatest difference is in the tin, which amounts to nearly $\frac{13}{10,000}$dths of an inch in excess; and it is more than probable that the absolute expansion of this metal has not hitherto been obtained with sufficient precision, and that it even varies in different states. I shall return to this subject in the second part of this paper, which I reserve for a future communication; in which I hope to be able to lay before the Society observations and tables of the dilatations of metals to their melting points. It is my intention in this first part to touch no further upon the subject of expansion than is sufficient to establish confidence in the pyrometer as a measure of heat.

Another confirmation of the precision of these observations may be derived by calculating the expansion of the black-lead register

register for the 150°, from the greater expansion previously determined by the boiling point of mercury for

$$596° : \cdot 00831 :: 150° : \cdot 00209$$

which only differs $\frac{25}{100,000}$dths of an inch from the above mean.

Exp. 4. It was a principal object to ascertain whether any and what difference existed in the expansion of different specimens of the black-lead earthenware: two or three registers which I had cut out of the same crucible gave me almost identical results by exposure to boiling mercury. I then selected another specimen by a different manufacturer. Its grain was very fine, and its texture more close and compact than the former. It was twice exposed with the platinum bar to boiling mercury. The first time it was boiled for a quarter of an hour, and the arc measured was 1° 45′. The second time the boiling was continued for only ten minutes, and the reading was precisely the same. The expansion was therefore ·01526.

Absolute expansion as before......	·01947
Apparent expansion	·01526
Expansion of black-lead...........	·00421

Exp. 5. The same register of the fine-grained black-lead was exposed for a quarter of an hour with the iron bar to boiling mercury: the arc measured on the scale was 2° 49′ = expansion ·02457.

Absolute expansion as before.........	·02914
Apparent expansion	·02457
Expansion of black-lead...............	·00457
Fine-grained black-lead by platinum	·00421
by iron......	·00457
Mean...	·00439

the two experiments differing from the mean by less than $\frac{2}{10,000}$dths of an inch. This shows that the fine-grained ware expands less than the coarser, and proves the necessity of ascertaining the expansion of each register for itself by boiling in mercury; at least till some means be taken to insure their uniform composition. Every register should also be marked with a reference to its proper expansion; and I would recommend all those who may use the instrument for delicate researches, to verify this point for themselves; as they may easily do with the apparatus before described.

Exp. 6. The expansion of the last specimen of black-lead ware

ware being nearly the least which has fallen under my observation, I repeated with it the experiment of the dilatation of six of the former list of metals to the boiling point of water; as the accuracy of these observations is a point which it is of the greatest importance to establish.

The subjoined Table contains the results.

Table VIII.

Platinum ...	0° 22′	= ·00319	from 60° to 212°
Iron	0 39	= ·00566	———
Copper......	0 54	= ·00785	———
Brass	0 59	= ·00857	———
Gold.........	0 41	= ·00595	———
Silver.	0 58	= ·00843	———

The differences of the observed expansions and the real are also subjoined and ranged by the side of those obtained by the first series of observations.

Table IX.

	Expansion of the Metal Bars.		Expansion of Black-lead. Second Series.	Differ. from Mean.		First Series.	Differ. from Mean.
Platinum	absolute ·00478						
	apparent ·00319						
	———	=	·00159	·00000	...	·00202	—·00032
Iron	absolute ·00640						
	apparent ·00566						
	———	=	·00074	—·00085	...	·00132	—·00102
Copper ...	absolute ·00930						
	apparent ·00785						
	———	=	·00145	—·00014	...	·00247	+·00013
Brass.....	absolute ·01045						
	apparent ·00857						
	———	=	·00188	+·00029	...	·00246	+·00012
Gold......	absolute ·00794						
	apparent ·00595						
	———	=	·00199	+·00040	...	·00242	+·00008
Silver.....	absolute ·01034						
	apparent ·00843						
	———	=	·00191	+·00032	...	·00220	—·00014
		Mean	·00159	....		·00234	

The agreement of this second series with each other is quite as close as that of the first; and it is worthy of remark, that the greatest variation from the mean is in both cases with the iron in deficiency, and nearly to the same amount of one half. It is not unlikely, therefore, that there may be some error in estimating the absolute dilatation of this metal, which is probably something greater than we have assumed.

If we estimate the expansion for these 150° to the boiling point of water from the result obtained by the boiling of mercury, we shall have the following proportion:—

596° : ·00439 : : 150° : ·00110

 which

which does not differ quite $\frac{5}{10,000}$dths of an inch from the foregoing mean.

Having thus, I trust satisfactorily, established the accuracy of the pyrometer, and the degree to which confidence may be placed in its indications, I shall conclude this part of my subject with the details of some experiments upon the fusing points of different metals. I shall designate the registers of coarse and fine-grained black-lead respectively by the letters A and B.

Exp. 7. About 30lbs. of the clippings of thin sheet copper were very gradually melted in a crucible in the blast furnace of the Royal Institution. The platinum bar was adjusted in the register B, and when the metal was about half run down, it was placed perpendicularly with the index upwards in the crucible, and held down with a pair of tongs. The crucible was then gradually fed with the clippings till the melted metal covered about two-thirds of the register. In this situation it was kept ten minutes, and when it was lifted out some of the metal remained unmelted. A crust of oxide, mixed with metal, had also affixed itself to the upper part of the black-lead. This was partially dissolved away and loosened by immersing the register with great care, when cold, in a diluted mixture of sulphuric and nitric acids. The whole was thus easily removed, and the black-lead exhibited a perfectly clean surface. The arc measured upon the scale was 5° 49′, denoting an expansion of ·0508. The temperature of the laboratory was about 65°.

I am indebted to the kindness of Mr. Mathison for unexceptionable opportunities of taking the melting points of gold and silver at the Royal Mint, who also most obligingly assisted me in the operations. Two new registers were prepared, which I shall designate as II. and III.: their rates of expansion were not determined till after the experiments.

Exp. 8. The register II. was carefully adjusted with the platinum bar. About 90lbs. of fine gold were weighed, and one of the ingots was cut into ten pieces for the purpose of gradually feeding the crucible, and keeping the temperature down to the true melting point during the observation. The remainder was melted in a black-lead crucible in a wind-furnace. When just fused, one of the pieces was thrown in, and the melted metal immediately congealed upon the surface. The register, which had been slowly heated in another crucible to a dull red, was then taken up with a pair of tongs and plunged perpendicularly into the gold about two-thirds of its height. In this situation it was kept ten minutes, and during the time two more lumps of the metal were thrown in. It was then carefully

carefully lifted out and set apart to cool. Its surface was perfectly clean, only a few small globules adhering to it, which were easily removed. I may here remark, that stirrers of the black-lead earthenware are constantly used at the Mint for agitating the melted gold. The arc measured from this experiment was 6° 10′, equivalent to an expansion of ·0537. Temperature of the air about 65°.

Exp. 9. The register III. was fitted with the iron bar, and also heated to a dull red. The temperature of the melted gold was prevented from rising by constant feeding with the pieces; the crucible being never left without some portion unmelted. It was then plunged beneath the surface of the metal as in the preceding experiment, and held in that situation for ten minutes. The arc measured was 9° 2′, indicating an expansion of ·0787.

Exp. 10. The rates of expansion of the two last registers were determined by boiling them for ten minutes each in mercury. The results were as follow:

	Arc.		Expansion.
II. with the platinum bar	1°·50	=	·0159
III. with the iron bar	2°·38	=	·0229

Exp. 11. About 50lbs. of pure silver were melted in a black-lead pot: a little scum floated upon the surface, which appeared at first like drops of oil upon a basin of water. I was afterwards informed that the metal had been refined with nitre, and the dross was owing to the action of a little remaining potash upon the crucible. Two registers had been prepared for the platinum and iron bars; but the observations were lost from the same action upon their substance. They were so deeply corroded in a line which corresponded with the level of the fluid metal, as to render it impossible to apply the scale, with any certainty, to their surfaces.

Exp. 12. Two new registers were selected, whose rate of expansion was found by boiling in mercury to be equal; the arc in both cases being with the platinum bar 1° 20′ = ·0116. They were marked IV. and V.

IV. was adjusted with the platinum bar. An ingot of silver, which had been refined by cupellation, weighing about 35lbs., was placed in a black-lead crucible in a wind-furnace. When somewhat more than three-fourths were melted, the register, previously heated to a dull red, was plunged into it as before, and held down for ten minutes. When lifted out, its surface was found perfectly good, and the few adhering globules of metal were easily removed. When cool, the scale was applied, and the arc found to be 4° 10′ = expansion ·0363. Temperature of the air 65°.

Exp. 13.

Exp. 13. The iron bar was placed in the register V., and having been previously heated was plunged into the same pot of metal. The silver at first set about the black-lead and adhered to it in a large lump. At the expiration of ten minutes this was just melted off, and the instrument was raised out of the crucible in a perfectly clean state. When cool, the arc measured was 7° 24′ = expansion ·0645.

Exp. 14. I made several attempts at the Royal Institution to ascertain the melting point of cast iron; but owing to the large quantity of the metal necessary; to the difficulty of keeping the temperature steady by constant feeding; and to the failure of crucibles,—I did not succeed. I am under obligation to Mr. Parker of Argyle-street, for the readiness with which he afforded me every facility of performing the experiment at his foundry.

I selected a new register for the occasion, which was marked I. Its rate of expansion was not determined till after the experiment. A crucible was prepared capable of containing about 35lbs. of the metal. It was filled with pieces of the best gray iron, and placed in a powerful wind-furnace, which admitted of the operator standing immediately above the crucible with complete command over it. When the metal was melted, the crucible was lifted from the furnace, and the dross skimmed off its surface. It was then replaced; a lump of the same iron was thrown into it, and the register, previously heated red hot, was immersed in the fluid to about the same depth as in the former experiments. It was kept in this situation by means of a pair of tongs for ten minutes, and afterwards gently lifted out and laid upon hot sand. A thin scale of iron adhered to the black-lead, which when cold was easily removed, and retained the form of the bar like a sharp cast, and left the surface of the register perfectly clean and bright. The arc measured after the experiment was 6° 16′ = expansion ·0546. Part of the lump of metal remained unmelted.

Exp. 15. Another register, which had been prepared with the iron bar, was immediately immersed in the fluid metal. The fire, however, had been allowed to fall, and the iron almost instantly congealed; and in attempting to lift the register out, it was found to be set fast and broke. The experiment was so far instructive, that it proved how nearly the exact melting point had been attained in the preceding experiment. The iron bar was removed uninjured.

Exp. 16. The register I. with the platinum bar was boiled in mercury for ten minutes: the arc afterwards measured was 1° 20′ = expansion ·0116.

Exp. 17. About 30lbs. of zinc were carefully melted in a crucible

crucible set in a common fire, assisted with the bellows. The register A was prepared with the iron bar and held down in the metal, which was supplied from time to time so as to insure its very gradual fusion, and some portion always remaining in the solid state. In ten minutes time it was removed, and when cold the arc measured was 2° 45′, equivalent to an expansion of ·0239.

A dry stick of deal plunged into the melted metal for a few seconds caused a violent ebullition, and was deeply charred. The zinc in this state did not appear red in the light.

Exp. 18. About 12lbs. of zinc were melted in a smaller crucible: the register B prepared with the iron bar was immersed in it; but instead of being gradually supplied, the heat was allowed to increase after fusion till it began to burn: at this point there was an evident blush of red upon its surface. The arc measured upon this occasion was 4° 7′ = expansion ·0358.

[To be continued.]

XXXVI. *On the Calculation of the Orbits of Double Stars.* *By* Professor Encke.

[Continued from vol. ix. page 410.]

In making the corrections for the purpose of fulfilling the equations of condition, one may be guided by the consideration that

$$k = \varrho^2 \frac{dp}{dt} = \text{constant}.$$

If, therefore, several p have been observed at intervals of time not too great, but likewise not too small, so that we may put, approximately, $\frac{\Delta p}{\Delta t}$ in place of $\frac{dp}{dt}$; and if the distances appear to be more certain at one epoch than at another, one may correct the distances at the other epoch, having

$$\varrho_1 \varrho_1 \frac{\Delta p_1}{\Delta t_1} = \varrho_2 \varrho_2 \frac{\Delta p_2}{\Delta t_2}$$

We have already observed, that the angles of position appear to be capable of a more accurate determination than the distances. On account of the uncertainty of the observations of the angles of position, it will not be advisable to take Δt too small. As soon as one is pretty near the truth, and a rough drawing will, in most cases, lead us so far, these trials appear to be most adequate to the purpose on account of the simple form of the equation. In the beginning it will, however, be necessary

necessary to change α more frequently than is desirable, as $\frac{d\,\varphi\,x}{d\,x}\left(=4\sin x^2\right)$ will exceed 1 when x exceeds 30°.

This calculation may, however, be abbreviated, as the calculation of $a\,b$ may be saved by immediately substituting the values of (15) in the equations (16). Combining the value of $a\,b$ with one of the equations (16) in which the same differences of the excentric anomalies occur,—for instance, the third equation (15) with the first (16),—we obtain

$$k(t_2-t_1)-(0\ 1\ 2)=\frac{(1\,2\,3\,4)\ \text{tang}\ 2\,\zeta\ \text{tang}\ \zeta}{\text{tang}\ 2\,\zeta_1}\left\{\frac{2\,(\beta-\alpha)-\sin 2\,(\beta-\alpha)}{4\sin(\beta-\alpha)^2}\right\}\frac{1}{\sin 2\gamma}$$

consequently in every equation we shall have a function of this form:

$$\chi(x)=\frac{2\,x\ -\sin 2\,x}{4\sin x^2}$$

whose value may be given in a table.

The function may be reduced to the following form:

$$\chi(x)=\tfrac{1}{2}\,x\,(1+\ \text{cotang}\,x^2)-\tfrac{1}{2}\ \text{cotang}\,x$$
$$=\tfrac{1}{2}\,x+\tfrac{1}{2}\,(x\ \text{cotang}\,x-1)\ \text{cotang}\,x$$

We have now, if A, B, C stand for the numbers of Bernoulli, viz.

$$A=\tfrac{1}{6},\ B=\tfrac{1}{30},\ C=\tfrac{1}{42},\ \&c.,$$

these equations:

$$x\ \text{cotang}\ x-1=\ -A\frac{2^2}{1\,2}x^2-B\frac{2^4}{1\,2\,3\,4}x^4-C\frac{2^6}{1\,2\,3\,4\,5\,6}x^6\ \ldots\ldots$$

$$\text{cotang}\,x=\frac{1}{x}-A\frac{2^2}{1\,2}x-B\frac{2^4}{1\,2\,3\,4}x^3-C\frac{2^6}{1\,2\,3\,4\,5\,6}x^5\ \ldots\ldots$$

Having due regard to the following relations:

$$B=\frac{4\,3}{1\,2}\cdot\frac{1}{5}A^2$$

$$C=\frac{6\,5}{1\,2}\cdot\frac{2}{7}A\,B$$

$$D=\frac{8\,7}{1\,2}\cdot\frac{2}{9}A\,C+\frac{8\,7\,6\,5}{1\,2\,3\,4}\cdot\frac{1}{9}B^2,\ \&c.$$

we obtain for $(x\ \text{cotang}\ x-1)$ cotang x this series:

$$-A\frac{2^2}{1\,2}x+B\frac{2^4}{1\,2\,3}x^3+C\frac{2^5}{1\,2\,3\,4\,5}x^5+D\frac{2^7}{1\,2\,3\,4\,5\,6\,7}x^7\ldots\ldots$$

and

$$\chi(x)=\frac{2}{1}A\,x+\frac{2^3}{1\,2\,3}Bx^3+\frac{2^5}{1\,2\,3\,4\,5}Cx^5+\frac{2^7}{1\,2\,3\,4\,5\,6\,7}Dx^7\ldots.$$

By this series the value of the function may be easily calculated for small values of x; for greater ones the finite expression is accurate enough for calculation. The limit of the conver-

convergency of the series may be most easily investigated by introducing instead of A, B, C, &c. the sums of the reciprocal even powers of numbers; if

$$a = 1 + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} \ldots\ldots$$

$$b = 1 + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4} \ldots\ldots$$

$$c = 1 + \frac{1}{2^6} + \frac{1}{3^6} + \frac{1}{4^6} \ldots\ldots$$

it is well known that

$$a = \frac{2\,A}{1\,2}\,\pi^2$$

$$b = \frac{2^3\,B}{1\,2\,3\,4}\,\pi^4$$

$$c = \frac{2^5\,C}{1\,2\,3\,4\,5\,6}\,\pi^6$$

consequently,

$$\chi(x) = \frac{2\,a}{\pi}\cdot\frac{x}{\pi} + \frac{4\,b}{\pi}\cdot\frac{x^3}{\pi^3} + \frac{6\,c}{\pi}\cdot\frac{x^5}{\pi^5} + \frac{8d}{\pi}\cdot\frac{x^7}{\pi^7} + \ldots\ldots$$

In the distant terms of the series the ratio of two successive powers of $\frac{x}{\pi}$, say $\left(\frac{x}{\pi}\right)^{2\mu-1}$, and $\left(\frac{x}{\pi}\right)^{2\mu+1}$, whose coefficients let be represented thus:

$$\frac{2\,\mu}{\pi}m \text{ and } \frac{2\,\mu + 2}{\pi}n,$$

will more and more approach to unity, so that the series will always converge for $x < \pi$. Making $x = \frac{1}{2}\pi$ we have $\chi(\frac{1}{2}\pi) = \frac{1}{4}\pi$, consequently

$$\frac{1}{4}\pi^2 = a + \frac{2\,b}{2^2} + \frac{3\,c}{2^4} + \frac{4\,d}{2^6} + \frac{5\,e}{2^8} \ldots\ldots$$

Within the limits $x = 0$ and $x = \frac{1}{2}\pi$, the change of this latter function is only the fourth part of that of the quantity $2\,x - \sin 2\,x$. This change may, however, be still more diminished, and the transcendental function to be introduced may be made still more like a constant quantity by combining it with a divisor by which the first power is made to disappear. If we choose $\sin x$ for this purpose, we have $\frac{2\,x - \sin 2\,x}{4 \sin x^3}$ exactly the same quantity employed by Gauss in his *Theoria Motus*, for solving the problem of determining the elements

 from

from two *radii vectores*, and the inclosed angle. This quantity changes between $x = 0$ and $x = \frac{1}{2}\pi$ from $\frac{1}{3}$ to $\frac{1}{4}\pi$; and the introduction of this quantity would have the additional advantage that, if two equations were combined, in which $\beta-\alpha$ and $\beta+\alpha$, or the other similar combinations occur, the ratio of $\sin(\beta-\alpha)$ and $\sin(\beta+\alpha)$ is very simply represented by equations (9). As this advantage, however, disappears in the case of the other combinations, it appears more convenient to use tables for the function:

$$\psi(x) = \frac{2x - \sin 2x}{4x \sin x^2}$$

$$= 2A + \frac{2^3}{1\,2\,3}Bx^2 + \frac{2^5}{1\,2\,3\,4\,5}Cx^4 + \frac{2^7}{1\,2\,3\,4\,5\,6\,7}Bx^6 \ldots\ldots,$$

for $x = 0$ the value of this quantity is $= \frac{1}{3}$
for $x = 90$ .... $= \frac{1}{2}$,

and for the intermediate values the changes are nearly constant. A table for this expression has likewise been appended at the end. It gives the values for every ten minutes of x; and for the convenience of the calculation the logarithms of the values of the function divided by the number of minutes contained in the radius have been given, or

$$(18) \quad \log\psi(x) = \log\left\{\frac{2x - \sin 2x}{4x\sin x^2} \cdot \frac{1}{3437{\cdot}75}\right\}.$$

The value of x expressed in minutes of an arc will always have to be combined with these values.

This function being introduced, and making

$$(19) \quad \begin{aligned} (1\,2\,3\,4)\frac{\tan 2\zeta}{\tan 2\zeta_1} &= N \\ (1\,2\,3\,4) \qquad &= N_1 \\ (1\,2\,3\,4)\frac{\tan 2\zeta_2}{\tan 2\zeta_1} &= N_2 \end{aligned}$$

the equations (16) will assume this form:

$$(20) \quad \begin{aligned} k(t_2-t_1) &= N \quad \text{tang}\,\zeta\frac{(\beta-\alpha)}{\sin 2\gamma}\psi(\beta-\alpha) + (0\,1\,2) \\ k(t_3-t_1) &= N_1 \quad \text{tang}\,\zeta_1\frac{(\gamma-\alpha)}{\sin 2\beta}\psi(\gamma-\alpha) + (0\,1\,3) \\ k(t_3-t_2) &= N_2 \quad \text{tang}\,\zeta_2\frac{(\gamma-\beta)}{\sin 2\alpha}\psi(\gamma-\beta) + (0\,2\,3) \\ k(t_4-t_1) &= N_2\,\text{cotang}\,\zeta_2\frac{(\gamma+\beta)}{\sin 2\alpha}\psi(\gamma+\beta) + (0\,1\,4) \end{aligned}$$

$k(t_4-t_2)$

$$k\,(t_4-t_2) = \mathrm{N}_1 \,\text{cotang}\, \zeta_1 \frac{(\gamma+\alpha)}{\sin 2\beta} \psi\,(\gamma+\alpha) + (0\;2\;4)$$

$$k\,(t_4-t_3) = \mathrm{N} \;\; \text{cotang}\, \zeta \frac{(\beta+\alpha)}{\sin 2\gamma} \psi\,(\beta+\alpha) + (0\;3\;4)$$

in all which, if the numbers of the tables are made use of, the angles $\beta-\alpha$ &c. are to be expressed in minutes of an arc.

In applying them the process will be nearly as follows. The observed angles p &c. give a preliminary knowledge of the values of α, β, γ, which will be sufficient for taking the functions $\psi\,(\beta-\alpha)$ &c. from the table so accurately that their subsequent alterations will never be considerable, their values for an interval of 90° being confined between $\frac{1}{3}$ and $\frac{1}{2}$.

Making next,

$$(21) \qquad \begin{aligned} \frac{\mathrm{N}\,\text{tang}\,\zeta}{t_2-t_1}\,\psi\,(\beta-\alpha) &= c_0 \;, & \frac{(0\;1\;2)}{t_2-t_1} &= d_0\,, \\ \frac{\mathrm{N}_1\,\text{tang}\,\zeta_1}{t_3-t_1}\,\psi\,(\gamma-\alpha) &= c_1 \;, & \frac{(0\;1\;3)}{t_3-t_1} &= d_1\,, \\ \frac{\mathrm{N}_2\,\text{tang}\,\zeta_2}{t_3-t_2}\,\psi\,(\gamma-\beta) &= c_2 \;, & \frac{(0\;2\;3)}{t_3-t_2} &= d_2, \text{ \&c.} \end{aligned}$$

we obtain equations of this form:

$$(22) \qquad \begin{aligned} k &= c_0 \frac{\beta-\alpha}{\sin 2\gamma} + d_0 \\ k &= c_1 \frac{\gamma-\alpha}{\sin 2\beta} + d_1 \\ k &= c_2 \frac{\gamma-\beta}{\sin 2\alpha} + d_2 \text{ \&c.} \end{aligned}$$

In the first approximation the quantities c, &c. are considered as accurately determined, and that value of α is sought, which, with its corresponding β and γ, will satisfy two equations. Trials will soon effect this purpose. By means of these values the quantities c are corrected, and they will then be obtained with an accuracy which hardly requires any further correction. The new values of α, β, γ, thence deduced are now substituted in the third independent equation, with the view of ascertaining whether all observations belong to the same ellipsis. Small differences are then corrected by an alteration of the times. In the cases of greater differences the distances for the less accurate observations must commonly be changed, and then, indeed, the $\zeta\,\zeta_1\,\zeta_2$, and the areas of the triangles on which they depend, must be found by a new calculation.

The formulæ (A), (B), (C), (6), (8), and (20), which only

 are

are required, being so very simple, and the calculation being more than sufficiently accurate if conducted with five decimal places, the time required for the calculation is not very considerable. If cases should occur in which observations only for which x exceeds 90° could be combined, one might either directly calculate the formula, or use this form:

$$(23)\qquad \psi(x) = \psi(180-x) - \frac{90^\circ}{180^\circ - x} \cdot \frac{\text{cotang}\, x}{x}.$$

It appears to be most convenient to begin with trials of assumed values of α. We have agreeably to the above-given notation:

$$\alpha = \tfrac{1}{4}(E_4 - E_3) - \tfrac{1}{4}(E_2 - E_1).$$

It will easily be decided whether α is to be taken negative or positive, whenever it differs considerably from 0. If there is no semi-revolution between any two successive observations, α will always be $< 45^\circ$. The quantities $\beta \pm \alpha$, $\gamma \pm \beta$, being by their nature always positive, and at the same time $< 180^\circ$, if the period of the observations does not embrace a whole revolution the quadrants in which β and γ are to be taken, are consequently fully determined.

As soon as α, β, γ and then likewise $a\,b$ have been determined, only two of the equations (4) are required for determining a, b, and s, and for obtaining, consequently, the projected ellipsis in magnitude and position with regard to the star at rest. If, however, we have been at the trouble, which cannot well be dispensed with, of adapting all four observations to the same ellipsis, we may obtain these data more easily by a symmetrical combination of the same.

[To be continued.]

XXXVII. *New Method of Levelling the Axis of a Transit Instrument.* By J. Nixon, *Esq.**

In the subjoined figure, A is an achromatic object-glass, firmly secured within one of the pivots of the hollow axis of the transit instrument. a is a *stop* furnished with adjustable cross lines placed within the other pivot exactly at the sidereal focus of the object-glass A.

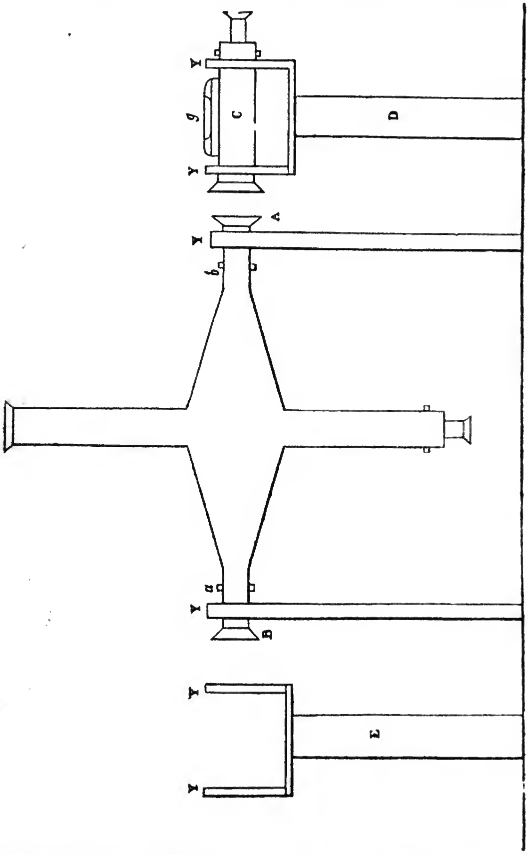
B is another object-glass fixed within the opposite pivot having its corresponding stop and cross lines situate at b.

D and E are two upright columns, each carrying a horizontal bar to which a pair of Ys is fastened. These columns might be attached to the Ys of the transit, but it would be more prudent to fix them independent of it.

* Communicated by the Author.

C is

C is an achromatic telescope fitted up with adjustable cross lines placed at the sidereal focus. It rests within the Ys of the support D, or those of the support E, according as it



is required to look with it through the object-glass A or B. (A vertical slip of thin mother-of-pearl, having a well-defined *dot* about the middle, would be better than cross-lines.)

The wires of the telescope C, and those within the axis of the transit, will require no other illumination than what is derived

rived from the transit lamp placed on the bar of the support opposite the one in which the telescope C rests during an observation.

To make the mathematical axis (or line of rotation) of the transit parallel to the horizon, the line of collimation of the object-glass A, and afterwards that of B, must be rendered parallel to the line of rotation. This will be the case when the line of collimation of the telescope C, placed against either object-glass, preserves during a revolution of the axis of the transit its parallelism to that of the object-glass to which it is directed. The line of collimation of both object-glasses being now parallel to the line of rotation, point the line of collimation of the telescope C exactly at that of the object-glass A, and note the level (*g*) attached to the former. Then remove the telescope to the Ys of the support E, and point it accurately at the intersection of the cross lines of the object-glass B. In the event of the line of rotation being horizontal, the bubble will revert to its previous position; otherwise half the deviation will be the inclination of the line of rotation.

As it may be difficult to make the line of collimation of the object-glasses rigorously parallel to the line of rotation, the telescope must be twice pointed at the object-glass; one observation taking place *before*, and the other *after* the axis of the transit has described half a revolution. The mean of the two readings of the level will then be equivalent to one reading with the cross lines *perfectly* adjusted.

In bringing the line of collimation of the telescope of the *transit* to be at right angles to the line of rotation, the latter must be levelled (after the method described) *before* as well as *after* the axis is reversed; otherwise, at least in the case of the pivots being unequal in diameter, the adjustment cannot be accurately effected.

When the line of collimation of each object-glass has been rendered parallel to the line of rotation, and the telescope C left correctly pointed at the cross lines of the adjacent object-glass, take the axis of the transit instrument out of its Ys, and replace it within them reversed in direction. The other object-glass will now be brought nearest to the telescope C; and should the line of collimation of the latter be found strictly parallel to that of the object-glass, it will prove that the pivots are of the same diameter.

Leeds, August 4, 1831. JOHN NIXON.

ERRATUM.—Vol. ix. page 428, line 37, *for* the angular opening of the Y, *read* half the angular, &c.

XXXVIII. *No-*

XXXVIII. *Notices respecting New Books.*

An Elementary Treatise on the Differential Calculus: comprehending the complete Theory of Curve Surfaces and Curves of Double Curvature. *By* J. R. Young.

HOW cursory soever be the survey we take of the present state of physical science, we must be convinced how extensively dependent it is upon the Differential and Integral Calculi, in some or other of their varied modifications. So complete, indeed, is this dependence, that he who attempts to pursue any one branch of physical inquiry beyond its simplest and most elementary state, will find his progress at once arrested, if he be not provided with this apparatus of research: and those who have penetrated most profoundly into the arcana of nature and of nature's laws, have uniformly concurred in the statement—that the principal desiderata are improved methods of effecting the mathematical processes to which the individual inquiries give rise. Several branches of philosophical investigation, indeed, seem to admit of no boundary, save that which the imperfection of our mathematics impresses on them; and every improvement which the calculus receives is sure to open new facts and new views, and often new paths of inquiry, which the mere experimentalist could never have suspected. The time may come, too, when sciences which as yet seem to acknowledge no discoverable quantitative laws, and especially with respect to the modification in the intensities of the producing forces, shall offer a splendid triumph to human perseverance, and fall as completely under the dominion of the symbolic methods, as the system of the celestial motions in the writings of Laplace can exhibit in our own time. This consummation is neither so visionary nor so remote as to discourage our hopes or paralyse our exertions: it rather, on the contrary, animates us with redoubled assiduity, whilst it offers the most cheering encouragements to future labourers. Already some gifted minds are intent upon marking the probable boundaries, and the proper route to be pursued; others are penetrating the dense and tangled masses of foliage, with which the baneful tree of error has overrun the soil, and intercepted our view of the recesses of nature's works; while others again, more humbly but not less usefully perhaps, stand at the entrance to encourage the young adventurer, and to point out to him the surest and the most successful route he can pursue. It falls to the lot of few to open new paths; but every man of science may more or less contribute to the ulterior object of all their exertion, and facilitate the progress of the young philosopher by removing the obstacles that would arrest his career. It is true, that in the capacity of elementary writers or scientific lecturers, all men will not be alike successful, whatever their actual attainments may be; for the peculiar faculties of the mind are as much displayed in the composition of an elementary book, in the verbal explanation of a process, or the illustration of a principle, as in any higher order of intellectual exertion. Higher, indeed, we have called it; but we are not sure that there is less loftiness of view, whilst we are certain that there is often much more

more logical clearness, displayed in a happy development of first principles, than the subsequent investigation commonly exhibits. At all events, it is a task in which there are more failures than are commensurate with the difficulty; except it be admitted that to produce a good elementary book be an undertaking of a higher order than is commonly supposed. In mathematical science this is peculiarly the case; and in that branch to which Mr. Young's book is devoted, this difficulty has been felt more strongly than, perhaps, in any other. The universal complaints which we hear of the unintelligibility, not to say inconclusiveness, of the processes and reasonings which stand at the threshhold of the Differential Calculus, must convince us that there is a radical defect somewhere, and stimulate our search for its place and its essential character.

Is there too wide a chasm between the *processes* of common algebra as now practised, and the elementary steps of the Calculus? Is there something in the leading *notions* which the science involves, which too greatly *transcends* the superficial conceptions of the undisciplined mind? or, finally, is there something in the doctrine itself *repugnant* to the conclusions which the ancient geometry, or the scarcely less satisfactory reasonings of the algebraic analysis has furnished examples of? With respect to its compatibility with the vulgar conclusions of "common sense," we might reply,—that the faculties of uneducated mind take cognizance only of general appearances of similarity, and neglect the more recondite differences, as well as the more recondite agreements that may subsist amongst the objects of its occasional contemplation; and therefore that its decisions are valueless on topics foreign to those it is conversant with, and even on its most familiar topics viewed under a novel aspect. To penetrate further than this, we must do more than perceive the individual facts, —we must class them, and reason on their appearances. This is the first step in science. To do this, however, we must make a vigorous effort to dethrone the host of prejudices which have usurped the place of reason, for it is no easy matter to reject the hasty generalizations of common sense, which, being acquired during the vagueness of early and desultory speculations, are almost always ranged on the side of error. Those processes of algebra, too, which are commonly studied prior to entering upon the Calculus, are too often modified to meet some fanciful or pertinacious objections of the man of mere common sense, rather than framed as an introduction to the *kind of thinking* which this science involves. It is presented as the end, rather than the beginning of mathematical science. Such a procedure might have been well adapted to Goldsmith's "loveliest village of the plain," and its interesting pedagogue; but as the first step towards those profound inquiries which characterize the physics of the present day, it is impossible to imagine how works could be worse adapted than our common treatises on algebra. This is the source of *much* of the difficulty that is felt in laying down the principles of the Differential Calculus; and considerable dexterity is required to enable the preceptor to lead his pupil to frame even a tolerable conception of the character of the science. Still this is not the only

only source; for after all, there is much vagueness (even amongst those who have become considerable adepts in the practice of the Calculus) in the *reasons* which are commonly offered for the validity of its processes. This vagueness is displayed in the preliminary steps of the inquiry in a striking degree. Amongst mathematicians of considerable eminence, we shall find confusion and contradiction prevail, as to the fundamental axioms, the fundamental definitions, and the fundamental reasonings of the science: and it almost invariably happens that the student acquires considerable practical dexterity long before he is able to unravel the mysteries of prime and ultimate ratios, of fluxions or *rates* of increase, or any of the other principles which have been made the basis of the method. If he have courage to persevere (more as a calculator than a metaphysician) till he can learn the method of operating;—if he can solve a few problems by means of the Calculus thus acquired; and if in addition he can verify these results by an appeal to some *geometrical* properties already known; then he learns, and *thus* he learns, by degrees, to feel confidence in the power and accuracy of his rules. He infers that *because* it gives right results in the cases he has tested, it *therefore* will in all. Yet *why* it should give right results in any one case, he cannot make out in any plausible manner: and, indeed, the vagueness and confusion which he experienced at the outset, when he attempted to master the *reasons*, discouraged and disgusted him too much to render it an agreeable undertaking to attempt the investigation anew. He thus willingly takes it upon trust, or on the authority which belongs to mere induction, instead of laying a broad and satisfactory basis upon a course of indisputable logical demonstrations. It is a painful fact,—but there is reason to fear that it is the case of nine mathematicians out of ten,—that they have no clear view of the first principles of the Differential Calculus, however expert they generally are in the application of its rules to the solution of problems: and it is in consequence of this painful fact, that we hail with pleasure the little work of Mr. Young, as a *panacea*, in some degree, for the evil we complain of. It is of the utmost importance that the young student should be inspired with *clear* views of the nature of the relations contemplated in the science: that his mind should be put in attitude to perceive the *force* of the reasoning—and that the ulterior objects of the Calculus should open upon his understanding with the utmost possible *distinctness*. This can only be effected by an author deeply in love with truth, and whose mind is enlarged by an extended survey of the subject upon which he writes;—an author who conducts his inquiries and develops his conceptions with due regard to method, and who is, moreover, endowed with a happy elegance of diction;—such a writer will not alter what has already been done well, for the mere purpose of differing from others, but will avail himself of the labours of his predecessors so far as they contribute to the great object he has in view. Yet he will always throw them aside when they are defective, illogical, irrelevant, or unnecessarily operose; and so modify them, where they admit of modification, as to alter the visual appearance, or even the received and familiarized phraseology, as little as is compatible with a clear and

perspicuous detail of the logic of the science, and the practice of the art. This is precisely the purpose by which Mr. Young seems to have been directed. There are no affected changes of method: there is no parade of original plan, or of novelty of principle; and yet there is much original matter, much original reasoning, and, what is of more value than all questions about originality in an elementary treatise, there is a perspicuity, a unity of method, prevailing in all its parts, that renders it, more than any book we have seen, *peculiarly adapted to instruction*. It is professedly composed "for the Use of Students in Schools and Universities;" and we think the science is brought more nearly to the level of school-boy capacities, than any work we have consulted brings it to the understanding of a University two-year man. We are persuaded that with due attention to the steps of explanation and demonstration, any student *with ordinary powers of mind*, and tolerably familiar with elementary algebra, may master the difficulties of the Differential Calculus by means of this work with very great facility; and this once effected, he will proceed to its diversified applications with a success that can never attend the labours of those who take first principles upon trust, and lean for the truth of the processes themselves, upon the authority of an imperfect evidence.

It is not, however, as an elegant and perspicuous development of the first principles of the Calculus, merely, that we have admired, and therefore recommended, Mr. Young's little work: we have found much to commend in it of a more profound character; much that we look for in vain in larger works, and indeed, in *all* English works.

We have already occupied so much of the room that we can devote to this notice, that we cannot possibly enter into particulars; and, indeed, we have greatly exceeded the limits of our plan, so that no apology will be expected from us if we refer those readers who are interested in this branch of mathematics, to the book itself, rather than transcribe it into our pages. We feel, however, that we should appear too abrupt to our readers and unjust to Mr. Young in closing without noticing one or two of his chapters. One is the discussion of the *failing cases of Taylor's theorem* (p. 98.): a chapter which offers a fair specimen of that happy facility in the discussion of an intricate and embarrassing question, which pervades the work, and which we think marks the perfection of didactic writing. This theorem, the mere accidental notice of which has conferred immortality on its author, is made the basis of almost all those mathematical processes employed in physical inquiry which in any way involve the Differential Calculus; and it has been aptly denominated by the late lamented namesake of the gentleman whose work we have now under review, (Dr. Thomas Young,) "a universal Solvent of Analytical difficulties." Whether this appellation be too strong or not, we shall not here inquire; but it has long been known, that as a general expression, it is subject to some limitations *in its application*. The knowledge that such limitations did exist, and our ignorance of any test by which their presence might be recognised, has long rendered the settlement of this question a matter of anxious interest to the mathematician and philosopher.

pher. Many explications and discussions have appeared at different times, more or less converging towards a correct theory of these cases; and we think the doctrine may be called complete as it appears in the work of Mr. Young *.

The doctrine of curves and curve surfaces is also developed with a neatness and elegance which we do not recollect to have seen in any English work; and though Mr. Young refers to Monge and Leroy as the authors whom he has principally had in view as models, yet upon looking into the work of the latter, we remark several great improvements in the execution; and as to that of the former, the portions which it could contribute to any elementary work are necessarily very few. We were particularly pleased with the demonstrations of the theorems of Euler and Meusnier upon the curvatures of surfaces, and with the somewhat novel way in which the evolutes of curves of double curvature are explained (p. 224.). Regard, however, must be had to a little correction on p. 225, as supplied in the Errata. We have remarked a few typographical errors in other parts of the work which it would be well to annex to the list already given, and which we hope the publisher will do. The paralogisms of some other writers, —distinguished ones too—are pointed out in the Preface, and in the body of the work; and many steps which have hitherto been deemed unquestionable, have been shown by Mr. Young to be altogether fallacious. We wonder, indeed, when we see them pointed out, why they did not occur to ourselves nor to anybody else till now; and we look upon the aptitude displayed in these detections to be highly characteristic of a mind which looks with a laudable anxiety to the purity of the fundamental principles of science.

The advantages furnished by the Syndics of the Cambridge University press in bringing out mathematical books, amounts almost to a monopoly; and the general prepossession of the public in favour of Cambridge men actually completes that monopoly, and shuts the door against all competition from mathematicians less advantageously situated †. Taking all circumstances into account, we doubt whether

* We would suggest to Mr. Y. that the remark at p. 106, needs no qualification, the same remaining true from $h = 0$ to $h = a$, both inclusive; for when $h = a$, although the expression becomes then

$$c = e + \sqrt{-a} \,.\, a^2 + \frac{1}{2\sqrt{-a}} \,.\, a^3 + \ldots.$$

it nevertheless continues true, because all the imaginaries on the right side will destroy each other.

Mr. Young has established, too, that when negative powers of h enter into the development, the terms which precede the first such power of h, is the true expansion: and it seems, we would hint, to depend on the same principle, viz. that all the *infinites* neutralize each other.

† "It is very easy to account for the appearance of more mathematical publications from Cambridge," says Dr. Abram Robertson, "than from any other place in this kingdom. The members of that University, who persevere in the study of science, and who are conscious of their ability either to elucidate its truths, or enlarge its boundaries, can look with confidence to the syndics of the press for assistance, to render their writings profitable to themselves, and useful to the public. All apprehension as to the pecuniary risk

ther the well-intended attempt of the founders of those funds to encourage the publication of valuable books has not been indirectly the cause of much injury to science, by shutting out from the contest all but a privileged body of competitors. It is like a statesman who shall offer a bounty to one manufacturer and refuse it to another: it is easy to see the consequences of the latter, and draw the parallel between the two cases. And its ill effect has been greatly aggravated by the enormous tax on paper, from which the University endowed presses enjoy an exclusive exemption, and the duty on advertisements, by means of which alone, an unknown or unpatronized author can bring his works before the public. We are led to make this remark by the following strain of just complaint in which the talented author, whose work has formed the subject of the preceding remarks, concludes his Preface, and with which we shall conclude our present notice.

"I am not, however, so sanguine as to look for much public encouragement of my labours, however successfully they may have been devoted: it is not customary to place much value, in this country, upon any mathematical production, of whatever merit, that does not emanate from Cambridge. The hereditary reputation enjoyed by this University, and bequeathed to it by the genius of Barrow, of Newton, and of Cotes, seems to have endowed it with such strong claims on the public attention and respect, that everything it puts forth is always received as the best of its kind. If this be the case with Cambridge books, of course it is also the case with Cambridge men, and accordingly we find almost all our public mathematical situations filled by members of this University. It is true that now and then, in the course of half a century, we find an exception to this; one or two instances on record have undoubtedly occurred, where it has been, by some means or other, discovered that men who had never seen Cambridge knew a little of mathematics, as in the case of Thomas Simpson, and of Dr. Hutton; but such instances are rare. It is not for me to inquire into the justice of this exclusive system; but, while such a system prevails, there need be little wonder at the decline of science in England: while all inducement to cultivate science is thus confined to a particular set of men, no wonder that its votaries are few. It is to be hoped, however, that in the present 'liberal and enlightened age,' such a state of things will not long continue, and that even the poor and unfriended student may be cheered up, amidst all the obstacles that surround him, in the laborious and difficult, but sublime and elevating career on which he has entered, by a well-founded assurance that his exertions, if successful,

risk of printing is removed; and those who write with a view to publish are at once stimulated by the laudable desire of fame, and animated by the hope of a fair and honourable recompense for their labour in the sale of their productions. In other places, *mathematicians may be equally successful in their attainments, and authors of treatises as worthy of public attention; but a wish to present them to the world is checked by the deterring consideration of an immediate and serious expense, and an uncertain and slow indemnification.*" —Reply to a Monthly and Critical Reviewer, p. 39.

will

will not be the less appreciated because they were solitary and unassisted."

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 16.—A PAPER was read, entitled, "A critical and experimental Inquiry into the Relations subsisting between Nerve and Muscle." By Wm. Charles Henry, M.D., Physician to the Manchester Royal Infirmary. Communicated by Wm. Henry, M.D., F.R.S.

It has long been a subject of controversy among physiologists whether muscular contraction is the immediate consequence of the action of a stimulus on the muscular fibre, or whether it is necessarily dependent on a change taking place in the nerve distributed to the muscle, and excited by the stimulus. This question, the author observes, is one which, from its very nature, is incapable of a direct solution, because the intimate connection of nervous fibres with every part of the muscles renders it impossible to distinguish on which of these classes of textures the impression of the stimulus is primarily made. The continuance of the motions of the heart after the destruction of the brain and spinal cord, and even after the entire removal of the heart from the body, has been adduced as an argument of the independence of the contractile property of the muscular fibre: but this argument the author considers as inconclusive, because the nervous fibres remaining in the heart, and expanded on the interior of its cavities, may still be capable of performing their usual functions, and act as the medium of excitation to the muscular fibres: an hypothesis strongly supported by the analogy of the voluntary muscles, which, though usually excited to action by changes taking place in the central portions of the nervous system, may yet, when removed from this influence, be made to contract by irritations applied to the trunks of the nerves that supply them.

As narcotic poisons act exclusively upon the nervous system, the author conceived that they might afford the means of eliminating the action of the nerves, and thus enable us to discover what share they contribute towards muscular contraction. On applying the empyreumatic oil of tobacco, or the hydrocyanic acid, to the sciatic nerves of a rabbit, he found that the functions of that part of the nerve which was in contact with the poison were destroyed, and that irritations applied to that part no longer excited contractions in the muscles. But when the portion which had been so affected was cut off, and the galvanic wire applied to that extremity of the nerves which remained attached to the muscle, contractions were produced. Similar results were obtained when the poison was applied directly to the brain. When, on the other hand, the poison was applied to mucous surfaces so as rapidly to extinguish life, the muscles throughout the whole body were paralysed, and lost all capability of being excited to contraction.

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The inefficacy of opium applied to the cardiac nerves in arresting the motions of the heart has often been alleged as a proof that those motions are independent of the nerves. But the author found on trial that a solution of opium injected into the cavities of the heart, or introduced into the intestine, immediately arrested the muscular actions of these organs.

These phenomena appear to the author to accord best with the hypothesis that the immediate antecedent of the contractions of the muscular fibre is a change in the ultimate nervous filament distributed to that fibre.

A paper was read, entitled, "Experiments on the Length of the Seconds' Pendulum; made at the Royal Observatory at Greenwich." By Captain Edward Sabine, of the Royal Regiment of Artillery, F.R.S.

The experiments described in this paper were made with the original convertible pendulum constructed by Capt. Kater, and employed by the author in Portland Place, in the year 1817; except that the tail pieces were removed, and the moveable weight dispensed with: and they were made on the vacuum apparatus established in the south-west angle of the Pendulum-room, the place assigned for it by the Astronomer Royal. Having had reason to suspect that the retardation of the vibrations of the pendulum performed in circular arcs, when the weight was above, was greater than that assigned by the formula commonly employed, the author first investigates the correction necessary to be applied from this cause. He next ascertains the reduction to a vacuum for the small residue of air which the apparatus still contained, or for the small portion which may have introduced itself by leakage. The alteration of rate for each degree of Fahrenheit is then determined to be 0.441, a quantity almost exactly the same as that which was deduced from a former inquiry. The result of the present inquiry is, that the vibrations of Captain Kater's pendulum, which at 57° were found to be 86069.1 are at 62°, 86066.9. At this latter temperature, the length of the seconds pendulum, in vacuo, would be 39.13734 inches. Tabular details of the experiments accompany the paper.

A paper was read, "On recrossed Vision; being the Description of a distinct Tribe of ocular Phenomena, supplementary to a Rationale of the Laws of Cerebral Vision, recently published." By John Fearn, Esq. Communicated by Captain John Grover, F.R.S.

The phænomena described in this paper, and which the author designates those of *recrossed vision*, are cases in which objects placed between and very near the eye, such as the two sides of the nose, appear on opposite sides of the sphere of vision: the object on the right side of the nose being seen to the left by the right eye, and that which is on the left of the nose being seen to the right by the left eye. These and other phænomena illustrative of the well-known law by which we estimate the position of objects with relation to the eye to be in a line drawn from its image in the retina through the centre of the eye, are considered by the author as requiring further explanation. Not satisfied with the theory of Berkeley, that the

the mind is guided by the perceptions received from the sense of touch, in interpreting the signs furnished us by the sight, the author proposes to explain these phænomena by an hypothesis of his own, which he states in the following words. "Over and above the gift of two external or cranial eyes, man has been by his adorable Creator endowed with an internal cerebral organ, which performs the office of a *third eye*, by being the common recipient of impressions propagated either from one, or both of the external eyes; and the mind, in her chamber of percipience, steers with regard to external objects by the same principle on which the mariner steers by his compass. Thus the two cranial eyes are analogous, in principle and situation, to two magnetic compasses placed upon a ship's deck; while the third, or cerebral eye, corresponds to another compass placed in the cabin below; and the mind, situated like the captain-mariner in his cabin, knows, from consulting the cerebral eye, on what point of direction the body is steering; although the mind no more perceives either any external object, nor yet any image in the cranial eye, than the mariner perceives (even in the vulgar sense of the word perceiving) the far-off land, or haven, towards which he is surely making his way."

A paper was read, "On the Thermostat or Heat Governor, a self-acting physical Apparatus for regulating Temperature;" constructed by Andrew Ure, M.D., F.R.S.

The principle of the instrument here described is the unequal expansion of different metals by heat. A bar of zinc, alloyed with four or five per cent. of copper, and one of tin, about an inch in breadth, one quarter of an inch thick, and two feet long, is firmly and closely riveted along its face to the face of a similar bar of steel of about one third in thickness. The product of the rigidity and strength should be nearly the same, so that the texture of each may pretty equally resist the strains of flexure. Twelve such compound bars are united in pairs by a hinge joint at each of their ends; having the zinc or alloy bars fronting one another. At ordinary temperatures these bars will be parallel, and nearly in contact; but when heated, they bend outwards, receding from each other at their middle parts, like two bows tied together at their ends. When a more considerable expansion is wanted, a series of such bars is laid one over the other. The movement thus resulting is applied by the author in various ways to regulate the opening of dampers, letting in either cold air or cold water, or closing the draught of a fireplace, as the case may be. He proposes its employment to regulate the safety valves of steam boilers, as working with more certainty than the common expedients.

A paper was read, "On the Determination of the Thickness of solid Substances, not otherwise measurable, by Magnetic Deviations." By the Rev. William Scoresby, F.R.S. Lond. & Edin. Corresponding Member of the Royal Academy of Sciences of Paris, &c.

In the first part of this paper, the author states the results of a series of experiments undertaken by him with the view of ascertaining

ing whether all bodies are equally and uniformly permeable to the magnetic influence. Out of a great number of substances not ferruginous, but of various qualities, thickness, and solidity, which were subjected to trial, no instance occurred of their offering any perceptible obstruction to the action of a magnet on a compass, when interposed between them. No interruption to this action occurred even when the intervening bodies were iron ores, of which several were tried, excepting in one or two cases in which the ore was found to be itself magnetic. Hence the author was led to conceive that an accurate estimation of the magnetic influence transmitted through solid substances, might afford an excellent mode of ascertaining the thickness of such substances which might not be otherwise determinable. In order to judge of the degree of accuracy with which this might be accomplished, he instituted various sets of experiments; first placing the magnet in a line pointing to the centre of the compass, and on a level with it, in the east and west magnetic direction; and secondly in positions more or less oblique to this direction. He found reason to conclude from these trials, that the degree of accuracy attainable by this method was such as to render it highly advantageous in mining operations. Thus the thickness of a mass of freestone rock on the Liverpool and Manchester rail-way, three feet two inches in thickness, was determined by this method to within the eighth of an inch of its actual measurement, exhibiting an error of only one 334th part of the whole.

Many experiments were made to determine the effect which the form, dimensions, quality, and number of magnets have on the extent of their directive influence on the compass. It was found that little, if any augmentation of power results from increasing the thickness of the magnet; but that, with magnets of similar form, the directive forces are nearly in the direct ratio of their lengths. The author gives the results of an extensive series of experiments on the combined influence of several magnets, arranged, either in contact or in juxta-position, in a great variety of ways. The contact of dissimilar poles was in all cases productive of an increase, and that of similar poles of a diminution of efficiency.

In the second part of this paper the author enters into an investigation of the law of the magnetic directive power with reference to distance: in which he finds it convenient to estimate all distances in multiples of the length of the magnet employed, or, more correctly, of the interval between its two poles. From the established law of magnetic force,—namely, that it is in the inverse duplicate ratio of the distance,—the author deduces formulæ for estimating the directive power of a magnet on a compass at different distances. The combined action of four magnets, on a compass of Captain Kater's construction, which was five inches in diameter, will afford a tolerably accurate measurement of the thickness of any solid intervening substance, when about forty feet thick; but even at the distance of eighty-two feet the deviation produced by the magnet will be two minutes of a degree, and therefore still very appreciable. But the sensibility of the compass to the magnetic influence might be much further

further increased, by the application of a small directing magnet, placed in such a situation as to neutralize the greater part of the directive influence of the earth. By this means the author obtained a deviation in the compass of about 5′, at a distance of 61 feet, which extended through a variety of solid materials including soil, stones, and brick-work.

In the third part of this paper the author treats of the practical application of the magnetical influence in engineering, in tunneling, and in mining, for determining the thickness of solid masses in different situations where circumstances preclude the possibility of direct measurement. He adduces a variety of instances in which the information thus obtained would prove of the greatest value, in directing the operations in progress, or determining those to be undertaken, and frequently in preventing the occurrence of accidents which the want of such knowledge may occasion. He concludes with a statement and explanation of various practical directions for the employment of the method recommended.

A paper was read, "On a new Register-Pyrometer for measuring the Expansions of Solids." Part II. By J. F. Daniell, Esq. F.R.S., Professor of Chemistry in King's College, London.

In this paper, which is a sequel to that published in the Philosophical Transactions for 1830, the author prosecutes the series of experiments he had commenced on the dilatation of the metals: pursuing the comparison between the results of the experiments of Dulong and Petit, with those given by his own instrument. He finds a striking accordance between them in the case of copper, as he had already done with respect to iron and platina. He gives the result of some trials which he made with a view to obtain registers of uniform composition, so as to preclude the necessity of determining the rate of expansion in each individual instance. The results of his experiments on the dilatation of the metals are given in tables; the first showing in arcs of the scales the expansions of four metals from 62° to 212°, and thence to 662° of Fahrenheit; and their respective melting points: and the second, exhibiting the expansion of certain alloys to the same points. The experiments on the melting point of cast iron give a mean of 2768°, and present a remarkable coincidence with the corrected temperature deduced from the expansion of a platina bar, plunged into melted cast iron, which was 2786°; thus affording a conclusive proof of the accuracy of the pyrometer, and of its competency to determine fixed and comparable points of very high temperature. The author accordingly thinks himself warranted in recommending the introduction of the instrument extensively in all arts and manufactures, where it is an object to regulate high temperatures, and where it is calculated to determine many questions of the highest importance both to practical and theoretical science.

Two papers were read; the one entitled, "On the Influence of Screens in arresting the Progress of Magnetic Action:" the other, "On the Power of Masses of Iron to control the attractive Force of a Magnet." By William Snow Harris, Esq. F.R.S.

The object of the first paper is to show that every substance susceptible of magnetism by induction, when interposed as a screen, tends to arrest the action of a magnet upon a third substance: this intercepting power being directly as the mass and inversely as the susceptibility to induced magnetism. Thus, although a single plate of iron, about the sixteenth of an inch thick, effectually intercepts the action of a revolving magnet on a disc of copper, the same result is not obtained when the disc acted upon is also of iron, instead of being of copper; unless the mass of iron interposed be very considerable. The screening influence he found to depend on the mass of iron that is interposed, and not on the surface merely. He was led to suspect that a similar effect might be obtained by employing substances not of a ferruginous nature, provided they were interposed in considerable masses, and the result of his trials justified his conjecture. An account is given of several experiments made with large masses of silver, copper, or zinc, of about four inches in thickness, which being interposed between a revolving magnetic plate and a delicately suspended disc of tinned iron, completely intercepted the action of the magnet on the iron.

The author considers this interceptive property to be more or less common to every class of substance; and that in order to render it sensible, it is only necessary to employ the bodies in masses, bearing some direct ratio to their respective magnetic energies. Thus lead, having a weaker magnetic energy than copper, must be employed in a larger mass in order to produce an equal effect; and to render the screening power of ice sensible would require it to be above thirty feet in thickness. If, instead of interposing the screen of iron immediately between the revolving magnet and the suspended disc of copper, the iron be brought very near the under surface of the magnet, a similar neutralizing influence is exerted.

In the second paper, the investigation of this subject is resumed, and the neutralizing power of a mass of iron investigated under different circumstances. From the experiments detailed by the author, he is led to infer that substances highly susceptible of receiving transient magnetism, are the most efficient in their operation as screens; this operation being referrible to their neutralizing power. It is, however, very difficult to render this power sensible in the case of non-ferruginous bodies, unless they be actually placed between the magnet and the substance acted upon, so as to neutralize effectually the actions of those points which are nearest to each other. The attractive force exerted between a magnet and a mass of iron he finds to be always in the direct ratio of this controlling or screening power of the iron, or, in other words, to its neutralizing power in similar circumstances.

The author suggests that a temporary magnetic state may be conceived to be induced in a substance in two ways: either by the immediate action of the magnet upon each individual particle of the given substance, or else by the action of each particle of that substance on the next in succession, producing a propagation of magnetism from the one to the other. It may also, however, take pla

in both these ways at the same time. But these different modes of action appear to be in some inverse ratio of each other: for when the retentive or absorbing power of the substance is considerable, the power of the magnet becomes soon controlled; because the particles of the substance first acted upon, begin to operate as screens to the succeeding ones, and the induced magnetism, after a certain point, proceeds entirely by communication from particle to particle, until the whole power is expended. When, on the contrary, the retentive power of the given substance is small, little or no screening energy exists between its particles, in which case the magnetic excitement will depend upon the influence of the magnet on each individual particle: hence it is only by the succession or multiplication of effect resulting from a great number of particles, that we at length render the controlling power of such a substance sensible. The diminished action of a magnet on a disc of copper, when intersected by radiating grooves, seems to be owing to this cause, since a portion of the substance, requisite to the full development of the magnetic energy, is removed. In confirmation of this reasoning it was found that the number of oscillations of a delicately suspended bar, made in vacuo, in a given arc, surrounded by a mass of copper formed into rings, did not sensibly differ when, in the one case, that mass was made up of concentric rings, and, in the other, was entirely solid: while, on the contrary, by removing a very thin external lamina from the former, the number of vibrations was sensibly changed.

The concluding part of this paper is occupied by speculations on the nature of magnetic action: the author being disposed to regard a magnet as rather in a passive than an active state, when exhibiting the phænomena of magnetic attraction. This attraction he considers as the result of an impression first made on the magnet by the iron which appears to be attracted by it: because he finds that with different masses of iron of the same quality, the force at the same distance is unequal; being with some pieces very sensible, whilst with others it is altogether inappreciable. He views a magnet as a substance put into a peculiar state or condition, in consequence of which it exhibits certain properties when subjected to external excitation; in a way analogous to the elastic force of a spiral spring, which is not called into action unless that spring is stretched by a weight suspended to it, or by some other extraneous force. In the case of magnetism, the exciting substance is likewise affected in a similar manner with the magnet which it excites; and the analogy of the spiral spring may be further pursued, in order to render the two cases corresponding, by supposing the weight which elongates the first spring to be itself another similar spiral spring, which is also elongated while exerting its force on the first. Under these circumstances the separation of the coils will be greatest at the upper end of the whole combination of springs, and least at the lower part, presenting a contrariety of states at the two extremities, analogous to the opposite polarities of the two ends of a magnet.

A paper was read, "On the Atmosphere of Mars." By Sir James South, F.R.S.

The author refers the origin of the hypothesis of the "Extensive Atmosphere of Mars" to the observations of Cassini and Rœner, made at Briare and Paris in the year 1672. By the former it would seem that a star of the fifth magnitude became invisible with a three-feet telescope when at a distance of six minutes from the planet; whilst by the latter the same star, after having undergone occultation by the planet, could not be perceived with a large telescope till Mars had receded from it a distance equal to two thirds of his own diameter; although with the same instrument stars of similar magnitude might be easily distinguished even when in contact with the moon's limb.

As opposed to these observations, the author advances his own. One, dated Blackman-street, February 19, 1822, in which a star of the ninth magnitude as seen with the five-feet equatorial suffered no diminution of its apparent magnitude, at a distance of 103 seconds from the planet. A second, on the night following, when the star 42 Leonis having been seen within a second of a degree of the planet's limb prior to occultation by the planet, was perceived after emersion, when only one second and one tenth from it; the instruments of observation in this instance were the five-feet equatorial and the thirty-inch Gregorian reflector, the former instrument being used by the author, the latter by Mr. Henry South. The third was made at Campden Hill, on the 17th of March of the present year, with an eight-feet achromatic of six inches aperture; and in this the star 37 Tauri was with a power of 320 seen actually touching the planet's limb.—The star in neither instance suffered more diminution of brightness than might fairly be attributed to the diffused light of the planet.

From these observations, and the apparently contradictory ones of Cassini and of Rœner, the author of this paper infers, that the existence of the extensive atmosphere of Mars is a subject highly meriting further investigation.

He then directs attention to the fact that 37 Tauri was of a red colour when in contact with Mars; whilst 42 Leonis was under similar circumstances of a blue colour: and, from inferences dependent upon observation, states, that the apparent anomaly is easily reconcilable, and that an hypothesis is not wanted to account, on the occasion alluded to, either for the red colour of the one star, or the blue colour of the other.

A paper was read, "On the Inflexion of Light." By John Barton, Esq. Communicated by Davies Gilbert, Esq. V.P.R.S.

The design of the author in undertaking the experiments of which he gives an account in the present paper, is to carry on the investigation of the phænomena of the inflexion of light from the point at which it was left by Newton. He begins by examining these phænomena in their simplest form, comparing the appearance of the shadow of an opaque body on a screen of white paper at different distances, with the appearance it would exhibit if the

rays

rays passed by the edge of the body, without suffering any deviation from a rectilinear course. It is well known that, under these circumstances, the real shadow is broader than the geometrical shadow, indicating a deflexion of the rays from the edge of the intercepting body. By varying the distances at which the observations are taken, it is found that the rays are not bent at a sharp angle, but pursue a curvilinear course, the concavity of which is towards the shadow, the curve itself resembling an hyperbola. A luminous halo also appears beyond the shadow; the breadth of this halo agreeing accurately, at all distances, with the space which the penumbra should occupy, if the rays were not bent. The author thinks it impossible to reconcile the explanation of these phænomena given by Newton, with his own hypothesis concerning the action of solid bodies on light, as stated in the "Principia:" for, in that hypothesis, the rays passing nearest to the edge of an intercepting body are supposed to be bent towards the edge, as if attracted; whereas the explanation proceeds upon the supposition that they are bent from that body, as if repelled. The actual hyperbolic course of the rays is also inconsistent with that hypothesis, which would assign to them a parabolic path. It also appears that the breadth of the spectrum made by receiving the sun's rays through an aperture one tenth of an inch, or more, in width, is less than if the rays proceeded in straight lines; but if the aperture is very much diminished, the result is reversed, the real spectrum being broader than the geometrical spectrum.

The author conceives, that the whole of the observed phænomena will admit of explanation, by assuming that light consists of material particles, endowed with a power of mutual repulsion, in which case they would obey the laws of elastic fluids; and the course of the rays might admit of comparison with the motions of the particles of air, or other similarly constituted fluids, in flowing past an obstacle opposed to their progress. He shows how this hypothesis furnishes an explanation of the deflexion of the rays, and of the curvature of their path; and why that path resembles an hyperbola. He supports this theory by the analogy of the laws of heat, considered as the properties of a material fluid, with those of light; both exhibiting the phænomena of reflexion, refraction, and polarization. The author is inclined to believe that, besides the deflecting force, the presence of which is already established, there exists also an inflecting force, which bends some of the rays towards the intercepting body; and states a variety of considerations in support of this fact. He explains, on the same principles, the phænomena described by Newton under the appellation of *fits of easy reflexion and easy transmission*, which Dr. Young has explained on the undulatory theory, by the principle of interferences: but which may be considered as analogous to the alternating movements of elastic fluids striking against an opposing body, or entering by a narrow aperture; movements which, in air, give rise to vibrations constituting musical sounds.

The Society then adjourned over the Long Vacation, to meet again on the 17th of November. ZOOLO-

ZOOLOGICAL SOCIETY.

June 28, 1831. Rev. W. Kirby in the Chair.

Mr. Vigors exhibited, on the part of Captain Cook, specimens of several *Birds* recently presented by that gentleman to the Society, and also of some other *Birds* shot by him in the South of Europe, some of which were interesting on account of their rarity, and others with reference to the localities in which they were obtained. Among them was a specimen of the *Pica cyanea* (*Corvus cyaneus*, Pall.), a species not included by M. Temminck in the 'Oiseaux d'Europe', which had been killed by Captain Cook in Spain. There were also specimens of the *Falco tinnunculoides*; of the *Sturnus unicolor*, Marm., killed in Spain; of the *Lanius meridionalis*, Temm., a species referable to the genus *Collurio* as recently distinguished by Mr. Vigors; of the *Sylvia conspicillata*, Marm., killed in Spain; of the *Saxicolæ cachinnans* and *stapazina*, Temm., also killed in Spain; and of the *Fringilla domestica*, Linn., which is met with in great numbers in Spain, and consequently extends far beyond the southern limits assigned to the species by M. Temminck.

A collection of Birds presented to the Society by H. H. Lindsay, Esq. of Canton, were laid upon the table. They were accompanied by a letter from that gentleman to the Secretary, of the date of Jan. 25, 1831, stating that the collection had been formed during the summer of the previous year in the neighbourhood of Manilla, and adding some notes respecting the various species, as well as the names in the Tagallo or native language of the country. The collection consisted of about fifty-six species, fifty of which at least had not previously been in the Society's Museum, or in any other public collection in England.—Mr. Vigors pointed out the different species; and announced that a catalogue of them was in preparation, which would shortly be submitted to the Committee. In the mean time he characterized the following species.

HIERAX ERYTHROGENYS. *Hier. capite et corpore suprà, caudâ femoribusque intensè atris; gulâ, collo in fronte, corporeque subtùs albis; strigâ a rictu ad aures extendente rufâ; rostro albo, pedibus nigris.*

Statura *Hier. cærulescentis.*

BUTEO HOLOSPILUS. *But. supernè brunneus, subtùs brunnescenti-rufus; capite, fasciisque duabus remigum rectricumque fusco-atris; nuchâ et dorso, collo in fronte, pectore abdomineque toto, tectricibusque alarum maculis albis ocellatis, harum maculis diminutioribus.*

Staturâ tertiâ parte minor quàm *Buteo Bacha*; ei speciei simillima, differt tamen capite lævi, corporeque toto maculato.

CAPRIMULGUS MACROTIS. *Cap. intensè brunneus, rufo undulatus, corpore subtùs caudâque rufo fasciatis; capite aurito scapularibusque rufo-brunneis, fusco undulatim punctulatis nigroque notatis; torque jugulari albo ad nucham extendente rufo.*

Longitudo *corporis*, 15; *rostri* ad frontem $\frac{7}{8}$, ad rictum, $1\frac{1}{4}$; *alæ* a carpo ad apicem remigis 2dæ, $10\frac{1}{4}$; *caudæ*, 7; *tarsi*, $\frac{1}{2}$.

DACELO LINDSAYI. *Dac. corpore suprà brunneo, olivaceo et viridi nitente,*

nitente, guttis rufo-albidis notato, pectore abdomine crissoque albis, illorum plumis, medii abdominis exceptis, olivascenti-viridi marginatis; capitis pileo saturatè olivascenti-viridi, vittâ superciliari lazulina circumdato, deinde vittâ per oculos nigrâ, alterâque suboculari ferrugineâ marginato; gulâ juguloque ferrugineis; strigâ utrinque maxillari lazulinâ; remigibus fuscis; rectricibus omnibus ad apicem, duabus utrinque externis ad latera, ferrugineo notatis; rostro subbrevi.

Longitudo *corporis*, $10\frac{1}{2}$; *rostri*, $1\frac{3}{8}$; *alæ* a carpo ad apicem remigis 3tiæ, $4\frac{1}{8}$; *caudæ*, $4\frac{1}{2}$, *tarsi*, $\frac{1}{2}$.

Dacelo Lessonii. *Dac. corpore suprà brunneo, olivaceo et viridi nitente, albido guttato; capitis pileo saturatè olivaceo-viridi, vittâ superciliari cæruleo-viridi circumdato, deinde vittâ alterâ nigrâ marginato; collo in fronte corporeque subtùs albo, pectoris abdominisque plumis viridi-brunneo marginatis; strigâ utrinque maxillari viridi; remigibus fuscis; rectricibus omnibus ad apicem, tribus utrinque externis ad latera, ferrugineo notatis; rostro sublongo.*

Longitudo *corporis*, $11\frac{1}{4}$; *rostri* $1\frac{7}{8}$; *alæ* a carpo ad apicem remigis 3tiæ, $4\frac{1}{4}$; *caudæ*, $4\frac{3}{4}$; *tarsi*, $\frac{5}{8}$.

Muscicapa occipitalis. *Musc. corpore suprà pallidè lazulino, capite colloque splendidioribus; abdomine lazulino-albido; maculâ occipitali grandi, torqueque gracili jugulari, sericeo-atris.*

Longitudo *corporis*, $6\frac{1}{2}$.

Rhipidura nigritorquis. *Rhip. cinereo-grisea; corpore subtùs, rectricumque, duabus mediis exceptis, apicibus albis; fronte, torqueque jugulari nigris; remigibus rectricibusque fuscis.*

Longitudo *corporis*, 7.

Irena cyanogastra. *Ir. nigrescenti-cyanea; capite suprà, fasciâ tectricum alarum, uropygio, crissoque splendenti-cyaneis; collo in fronte, genis remigibusque atris.*

Staturâ *Irenæ Puellæ*, et simillima; differt abdomine caudâque cyaneis, haud nigris, dorso cyaneo haud lazulino, et rostri culmine plus elevato.

Oriolus acrorhynchus. *Or. aureo-flavus; vittâ a rictu per oculos extendente sinciputque obtegente latâ, remigibus totis, rectricumque basibus nigris; rostro flavo, culmine elevato.*

Longitudo *corporis*, 12; *alæ* a carpo ad apicem remigis 4tæ, 6; *caudæ*, $4\frac{1}{2}$; *tarsi*, 1; *rostri*, $1\frac{1}{4}$.

Psittacula rubifrons. *Psitt. viridis, subtùs pallidior; fronte, dorso imo, rectricumque tectricibus coccineis; remigibus caudâque viridi-fuscis, rostro subelongato rufo.*

Statura paullò major quàm *Psitt. Galguli.*

Picus spilolophus. *Pic. dorso alisque sanguineo-coccineis; subtùs sordidè albus, fuscescenti undulatus; capite colloque nigris, guttis albis maculatis; hujus maculis grandioribus; remigibus caudâque fuscis, harum pogoniis internis albo maculatis.*

Longitudo *corporis*, $11\frac{3}{4}$.

Picus modestus. *Pic. suprà ater, alis ad latera apicesque subrufescentibus; capite in fronte genisque obscurè coccineis, occipite, gulâ,*

gulâ, jugulo, colloque grisescenti-atris, plumis maculâ minutissimâ albâ ad apicem terminatis; rectricibus duabus mediis elongatis.

Longitudo *corporis*, 15; *alæ* a carpo ad apicem remigis 4tæ, 6; *caudæ*, 6; *tarsi*, 1; *rostri*, $1\frac{1}{2}$.

Lampromorpha amethystina. *Lamp. suprà splendidè amethystina; abdomine albo, fasciis viridi-amethystinis ornato; rectricibus lateralibus albo notatis.*

Longitudo $7\frac{1}{4}$.

This description is taken from a bird in the state of change, the amethystine feathers on the back, tail and breast, appearing partially through a ferruginous ground, but sufficiently numerous and defined to indicate the adult plumage. A younger bird in the collection has nearly the whole of the upper body ferruginous with an amethystine feather here and there breaking out. In a note appended to the description of the species, Mr. Lindsay states that the natives considered them of extremely rare occurrence.

Nycticorax Manillensis. *Nyct. suprà castaneo-rufa; collo in fronte, abdominis lateribus, femorum tectricibus, alarumque tectricibus inferioribus pallidiori-rufis; capite colloque suprà nigris, cristæ pennis longis pendentibus albis, apice nigro; pectore abdomine crissoque albis.*

Staturâ paulo major quàm *Nyct. Caledonica*, cui simillima; differt tamen colore cristæ, colli in fronte, tectricumque inferiorum alarum.

July 12, 1831. W. Yarrell, Esq. in the Chair.

Skins of numerous species of *Mammalia* obtained in Dukhun, (Deccan), East Indies, were exhibited by Major W. H. Sykes, Corr. Memb. Z. S. They were accompanied by a Catalogue of the *Mammalia* noticed by Major Sykes in Dukhun, which included also observations on the habits of each species, with occasional remarks on their rarity or abundance, on their geographical range, and on other interesting points connected with their history.

The following species were enumerated:—

Semnopithecus Entellus, F. Cuv. *Makur* of the Mahrattas.—Is found in large troops in the woods of the Western Ghauts; and is not venerated by the Mahratta people, nor do they object to its being killed.

Macacus radiatus, Geoff. *Waanur* of the Mahrattas.—Inhabits the woods of the Western Ghauts in small troops.

Pteropus medius, Temm. *Wurbagool* of the Mahrattas.—Is very numerous in Western India, and such variations are found in the colouring of different individuals in the same troop, that two or three species might be supposed to be included in it. Some individuals have a greater length of body ($14\frac{1}{2}$ inches) than is given to the *Pter. Javanicus* by Dr. Horsfield.

Nyctinomus plicatus, Geoff. (*Vespertilio plicatus*, Hamilton?)—This *Bat* bears a very close resemblance to Dr. Horsfield's *Nyct. tenuis.*

Rhinolophus Dukhunensis, Sykes.—*Rhin. suprà murinus, infrà*

infrà albido-brunneus: auribus capite longioribus: antibrachio corpus longitudine æquante.

This *Bat* belongs to the same section as Dr. Horsfield's *Rhin. insignis*, but differs from that species in being much smaller; in having the ears larger and more rounded; the nose-leaf with the upper lobe concave, ridged beneath and revolute above; and the front lobe oblong and notched in the centre. It differs from the *Rhin. crumeniferus*, Pér. and Le Sueur, (which is the *Rhin. marsupialis* of M. Geoffroy's lectures, and the *Rhin. Speoris* of M. Desmarest,) in being much smaller, this species having the fore arm nearly half as long again as the Dukhun bat. The upper nose-leaf also is much more produced, and finally the colour of the fur in this species is reddish. The fore arm of the *Rhin. Speoris* as figured is 2 inches 2 lines long, and the body and head 2 inches 2 lines. In the Dukhun species the fore arm is only the length of the body. Expansion of its wings 10 inches.

Sorex Indicus, Geoff. *Cheechondur* of the Mahrattas.—These troublesome and disagreeable animals are very numerous in Dukhun, but much more so in Bombay. The sebaceous glands in an old male were observed to be very large, and the odour of musk from them almost insupportable; while in an adult female the glands were scarcely discoverable, and the scent of musk very faint. The *Sorex Indicus* and *Sor. giganteus* are regarded by Major Sykes as specifically identical, he having killed them in the same room, and seen them frequently together.

Ursus labiatus, Blainv. *Aswail* of the Mahrattas.—In the skulls of many individuals of this species which he has examined, Major Sykes has never seen more than four incisor teeth in the upper and six in the lower jaw; the two centre teeth standing a little in front of the line of the rest. One individual, now in his possession, is so young that he does not conceive that the deficient incisors can have fallen out; nor is there any appearance of dentition having existed in the places which they should have occupied. He remarks that it might be deemed advisable therefore to remove this animal from the genus *Ursus*.

Lutra Nair, F. Cuv. *Juhl Marjur* or *Water Cat* of the Mahrattas.—The *Otter* of Dukhun differs only from the *Nair* in wanting the white spots over the eyes, in having a white upper lip, and in being somewhat larger.

Canis Dukhunensis, Sykes.—*Kolsun* of the Mahrattas.

Can. rufus, subtùs pallidior: caudâ comosâ pendente: pupillâ rotundatâ.

This is the *wild Dog* of Dukhun. Its head is compressed and elongated; its nose not very sharp. The eyes are oblique: the pupils round, *irides* light brown. The expression of the countenance that of a coarse ill-natured *Persian Greyhound*, without any resemblance to the *Jackal*, the *Fox*, or the *Wolf*, and in consequence essentially distinct from the *Canis Quao* or *Sumatrensis* of General Hardwicke. Ears long, erect, somewhat rounded at the top, without any replication of the *tragus*. Limbs remarkably large and strong in relation

 to

to the bulk of the animal; its size being intermediate between the *Wolf* and *Jackal.* Neck long. Body elongated. Between the eyes and nose, red brown: end of the tail blackish.

From the tip of the nose to the insertion of the tail 33 inches in length: tail $8\frac{1}{2}$ inches. Height of the shoulders $16\frac{1}{2}$ inches.

None of the *domesticated Dogs* of Dukhun are common to Europe.

The first in strength and size is the *Brinjaree Dog,* somewhat resembling the *Persian Greyhound* in possession of the Society, but much more powerful.

The *Pariah Dog* is referable to M. Cuvier's second section. They are very numerous, are not individual property, and breed in the towns and villages unmolested.

Amongst the Pariahs is frequently found the *Turnspit Dog,* long backed, with short crooked legs.

There is also a petted minute variety of the *Pariah Dog,* usually of a white colour and with long silky hair, corresponding to a common *Lap-Dog* of Europe; this is taught to carry flambeaux and lanterns.

The last variety noticed is the *Dog* with hair so short as to appear naked like the *Canis Ægyptius.* It is known to Europeans by the name of the *Polygar Dog.*

CANIS PALLIPES, Sykes.—*Landgah* of the Mahrattas.

Can. sordidè rufescenti-albidus; dorso nigrescenti ferrugineoque vario; pedibus totis pallidè ferrugineis: caudâ sublongâ pendente.

This is the *Wolf* of Dukhun. Its head is elongated, and its muzzle acuminated: a groove exists between the nostrils. Eyes oblique: *irides* yellowish bright brown. Ears narrow, ovate, erect; small for the length of the head. Tail pendent, thin but bushy, extending below the *os calcis.* General colour of the fur a dirty reddish white or whited brown. Along the back and tail very many of the hairs are tipped black, mixed with others tipped ferruginous. The tail ends in a black tip. The inner surface of the limbs, the throat, breast and belly, dirty white. Legs pale. From the ears to the eyes reddish grey, with a great number of short black hairs intermixed; from the eyes to the nostrils, light ferruginous. The fur from the *occiput* to the insertion of the tail is two or three inches long, gradually shortening as it approaches the sides; hence all over the body very short and lying close.

The description is taken from two three-parts grown animals.

Length from tip of nose to insertion of tail 35 to 37 inches; of the tail 11 to 12 inches; the hair extending two inches beyond the measurement.

These animals are numerous in the open stony plains of Dukhun; but are not met with in the woods of the Ghauts.

Canis aureus, Linn. *Kholah* of the Mahrattas.—*Jackals* are numerous in Dukhun. Major Sykes had in his possession at the same time a very large wild male and a domesticated female. The odour of the wild animal was almost unbearable. That of the domesticated *Jackal* was scarcely perceptible.

CANIS

CANIS KOKREE, Sykes.—*Kokree* of the Mahrattas.

Can. suprà rufescenti-griseus, infrà sordidè albus; caudæ comosæ apice nigro; pedibus rufescentibus: pupillâ elongatâ.

The *Fox* of Dukhun appears to be new to science, although it much resembles the descriptions of the *Corsac*. It is a very pretty animal, but much smaller than the *European Fox*. Head short; muzzle very sharp. Eyes oblique: *irides* nut brown. Legs very slender. Tail trailing on the ground; very bushy. Along the back and on the forehead fawn colour with hair having a white ring near to its tip. Back, neck, between the eyes, along the sides and half way down the tail reddish grey, each hair being banded black and reddish white. All the legs reddish outside, reddish white inside. Chin and throat dirty white. Along the belly reddish white. Ears externally dark brown, and with the fur so short as to be scarcely discoverable. Edges of eyelids black. Muzzle red brown. Length 22 and 22½ inches: of the tail 11½ to 12 inches.

Viverra Indica, Geoff., (*Viv. Rasse*, Horsf.) *Juwadee Manjur*, or *Civet Cat* of the Mahrattas.—There are two varieties of this species of *Viverra* in Dukhun; one inhabiting the woods along the Ghauts; the other the country eastward of the Ghauts. The former has the ground colour much grayer, and the lines more distinctly broken into spots. The other variety has a ferruginous tint, and the four black longitudinal lines or stripes on the sides of the neck are more marked: it attains the length of 28½ inches.

Herpestes griseus, Desm. *Moongus* of the Mahrattas.—Some specimens of this animal measure from 19¾ to 20½ inches from the tip of the nose to the insertion of the tail, and the tail 15 to 16½ inches.

Paradoxurus Typus, F. Cuv. *Ood* of the Mahrattas.—This animal is by no means rare in Dukhun. Its carnivorous propensities are very strong, but it may be fed entirely on rice and clarified butter. In the stomachs of some individuals examined at Poona, were found fruit, vegetables, and *Blattæ*.

Hyæna vulgaris, Cuv. *Turrus* of the Mahrattas.—*Hyænas* are numerous in Dukhun, and are susceptible of the same domestication as a dog.

Felis Tigris, L. *Puttite Wagh* or *striped Tiger* of the Mahrattas.—Royal tigers are so numerous in the province of Khandesh that 1032 were killed from the years 1825 to 1829 inclusive, according to the official returns. They are much less numerous in the collectorates of Poonah, Ahmednuggar, and Dharwar.

Fel. Leopardus. *Cheeta* of the Mahrattas.—This is regarded by Major Sykes as the *Leopard* of M. Temminck's monograph of the genus *Felis*. It is a taller, longer, and slighter built animal than the succeeding, which he considers as the *Panther*. It differs also in more of the ground colour being seen, in the rose spots being much less curved, and in other particulars. The natives of Dukhun consider the *Cheeta* and succeeding Cat as distinct animals. The *Cheeta* is extremely rare. On the contrary, the

Fel. Pardus, Beebeea Baugh of the Mahrattas, is so abundant that

472 were killed from 1825 to 1829 inclusive, in the four collectorates of Dukhun. It exactly resembles the animal figured as the *Panther of the ancients* in Mr. Griffiths's Translation of the 'Règne Animal.' It differs from the preceding in its smaller size, stouter make, darker ground colour, and in its crowded rose rings.

Fel. jubata, L., and *Fel. venatica*, H. Smith. *Cheeta* of the Mahrattas.—These animals appear to be identical, the specific differences deduced from the hair originating in domestication. A skin of the wild animal has a rough coat, in which the mane is marked, while domesticated animals from the same part of the country are destitute of mane and have a smooth coat.

Fel. Chaus, Guld. *Mota Rahn Manjur* or *larger wild Cat* of the Mahrattas.

Fel. torquatus, F. Cuv. *Lhan Rahn Manjur* or *lesser wild Cat* of the Mahrattas.—The specimens from Dukhun differ only from the *Fel. torquatus* figured in the third volume of the 'Histoire Naturelle des Mammifères in the ears externally being tipped dark brown, and in having two narrow stripes behind the eyes instead of one. The sexes resemble each other in colour, marks and size.

Mus giganteus, Hardw. *Ghoos* of the Mahrattas.—In fully grown individuals of the well-known *Bandikoot Rat*, none of the teeth are tuberculous. Its body attains a length of $16\frac{7}{10}$ inches; the tail $11\frac{6}{10}$ inches.

Mus decumanus, Pall. *Chooa* of the Mahrattas.—The *Norway* or *brown Rat* abounds in Dukhun.

Mus Musculus, L.—The *Mouse* is comparatively rare in Dukhun.

Another *Mouse* was observed by Major Sykes, which he believes to be new. It is bright light chestnut above, reddish white below. Tail much longer than the body: size of the common mouse. Found only in fields and gardens.

Sciurus Elphinstonii, Sykes.—*Shekroo* of the Mahrattas.

Sc. suprà nitidè castaneus, infrà rufescenti-albidus; caudæ dimidio apicali pallidè rufescente.

This very beautiful animal is found only in the lofty and dense woods of the Western Ghauts. It is of the size of the *Sc. maximus*, and the general arrangement of its colours is the same; but its colours are invariable, and do not present those differences which exist in the *Sc. maximus*.

Ears and whole upper surface of the body, half way down the tail, outside of the hind legs and half way down the fore legs outside, of a uniform, rich reddish chestnut. The whole under surface of the body, from the chin to the vent, inside of limbs and lower part of fore legs, crown of the head, cheeks and lower half of tail, of a fine reddish white, the two colours being separated by a defined line and not merging into each other. Feet of a light red. Forehead and down to the nose reddish brown, with white hairs intermixed. *Irides* nut brown. Ears tufted. Length from the tip of the nose to the insertion of the tail 20 inches; of the tail $15\frac{1}{4}$ inches.

Dedicated to a very distinguished person and a zealous promoter of scientific research, the Hon. Mountstuart Elphinstone.

Sc.

Sc. Palmarum, Briss. *Khurree* of the Mahrattas.—The *Palm Squirrel* is very abundant in gardens in Dukhun.

Hystrix leucurus, Sykes.—*Sayal* of the Mahrattas.

Hyst. caudâ albâ.

This animal appears to be distinct from the European species, which it closely resembles in form and covering. It is nearly a third larger. All the spines and open tubes of the tail are entirely white, which is not the case in the *Hyst. cristata*. The spines of the crest also are so long as to reach to the insertion of the tail. The ears are much less rounded, and the nails are shorter, infinitely deeper and more compressed, and with deep channels below. The white gular band is more marked; and, finally, the Asiatic species is totally destitute of hair, spines where wanting being replaced by strong bristles even down to the nails.

Lepus nigricollis, F. Cuv. *Sussuh* of the Mahrattas.—This species of *Hare* is very common in the stony and bushy hills of Dukhun.

Manis pentadactylus, L. *Kuwlee Manjur* or *tiled Cat* of the Mahrattas.—Very common in Dukhun, living on white ants.

Sus Scrofa, L. *Dookur* of the Mahrattas.—*Wild Hogs* are numerous in Dukhun, and the males attain to a very great size. Every village also abounds with *Hogs*, but any property in them is equally abjured by individuals and the community. These *village Hogs* are of the same colour as the wild animal, mostly a rusty black, and the only variations are slate black or slate intense brown; but it is not above two thirds of the size of the latter. Tail never curled or spirally twisted.

Equus Caballus, L. *Ghora* of the Mahrattas.—A fine breed of *Horses* exists on the banks of the Beema and Mahn rivers in Dukhun, supposed to have been improved by the Arabian blood. The variety called *Pony* by us, and *Tuttoo* by the Mahrattas, is sedulously propagated.

Equus Asinus, L. *Gudha* of the Mahrattas.—The *Ass* of Dukhun is very little larger than a good mastiff or Newfoundland dog. It is said to be found wild in Katteewar.

Camelus Dromedarius, L. *Oont* of the Mahrattas.—The *Dromedary* is rarely bred in Dukhun, but is in very general use. The two-humped *Camel* is not known.

Moschus Meminna, Erxl. *Peesoreh* of the Mahrattas.—This beautiful little animal is found in considerable numbers in the dense woods of the Western Ghauts, but never on the plains.

Cervus equinus, Cuv. *Sambur* of the Mahrattas.—Abounds in the Ghauts of Dukhun and in Khandesh, and is no doubt the same as the Malayan *Rusa* figured in Mr. Griffiths's Translation of the 'Règne Animal'. It wants the size of the *Cerv. Aristotelis* of Bengal, also called *Sambur* (not *Samboo*), and is not so dark in colour.

Cerv. Muntjak, Zimm. *Baiker* of the Mahrattas.—This beautiful species of *Deer* is a native of the Western Ghauts of Dukhun, and is never seen on the plains. It has large suborbital sinuses, which it uses in the manner of the *Ant. Cervicapra*.

Antilope Cervicapra, Pall. *Bahmunnee Hurn* of the Mahrattas.—This animal abounds on the plains of Dukhun, in flocks of scores, but

but is not met with in the Ghauts. The suborbital sinuses are capable of great dilatation, and the animal applies them to objects as if for the purpose of smelling.

ANT. BENNETTII, Sykes. *Ant. cornubus nigris, lyratis, apicibus lævibus leviter introrsum antrorsumque versis, ad basin ultra medium annulatis (annulis 8–9); rufescenti-brunneus, infrâ albus, fasciâ laterali haud conspicuâ; fasciâ mediâ strigâque ab angulo oculi ad oris angulum extensâ nigris; caudâ nigrâ.*

Kalseepee or *Black Tail* of the Mahrattas. *Goat Antelope* of Europeans.

This *Antelope* is found on the rocky hills of Dukhun, rarely exceeding three or four in a group, and very frequently solitary. It belongs to the same section as the *Ant. Dorcas*. Horns erect, slightly diverging from each other, bending slightly backwards at first, subsequently with their points bending forward: ringed for $\frac{2}{3}$ of their length. The whole upper surface and outside of the limbs rufous or red brown. Under surface and inside of the limbs white. Tail black. A black patch on the nose. A black narrow streak from the anterior corner of each eye towards the angle of the mouth. Suborbital sinuses very small; in dried skins not observable; nor does the animal dilate them unless very much alarmed. Limbs long and slender; black tufts at the knees. Body light. The female has horns, but they are slender, cylindrical, and without rings. The buttocks present a heart-shaped patch of white. Unlike the *Ant. Cervicapra* it carries its tail erect when in rapid motion. It stands as high as the *Bahmunnee Hurn*, but has less bulk.

There is another *Antelope* found in Dukhun, which Major Sykes has not yet identified, on account of the immature age of his specimen. It is brown above, whited brown below. Horns cylindrical, pointed, without rings. Its general appearance is that of the *Ant. rufescens* and *Ant. silvicultrix*.

Capra Hircus, Linn. *Bukee* of the Mahrattas.—The *goats* in Dukhun are gaunt, stand high on their legs, have the sides much compressed, and are covered with long shaggy hair, which in most is black. Ears nearly pendent. *Irides* ochrey yellow or reddish yellow. Tail always carried erect in movement.

Ovis Aries, Linn.—The variety of *Sheep* most extensively bred in Dukhun, has short legs, short thickish body, and arched chaffron. The wool is short, crisp and coarse, and is almost universally black. In most individuals there is a white streak or line from the anterior angle of each eye towards the mouth, and a white patch on the crown of the head.

Ant. picta, Pall. *Damalis risea*, H. Smith. *Rooee* of the Mahrattas. *Nylghaū* of the Persians.—This animal is an inhabitant of the Western Ghauts of Dukhun.

Bos Taurus, var. *Indicus*. (*Bos Indicus*, Linn.) *Pohl* and *Byl* of the Mahrattas.—This animal, remarkable for its hump, is when early trained to labour or to carriage nearly destitute of it. Dwarf cattle are not met with in Dukhun.

Bos Bubalus, Br. Male called *Tondgah;* Female, *Muhees* of the Mahrattas.

Mahrattas.—The *Buffaloe* of Dukhun is the long-horned variety, and is mostly bred in the Mawals or hilly tracts along the Ghauts.

Major Sykes subsequently called the attention of the Committee to a *Monkey* presented by him to the Society, and now living at the Gardens. It was obtained at Bombay, where it was believed to have been taken from Madagascar; and as it has some characters in common with the *Cercopitheci* (especially with the group of which the *Cerc. Sabæus* forms a part) and the *Semnopitheci* of India, it was remarked that it may ultimately prove to be a connecting link between the African and Asiatic monkeys. It wants the long limbs of the *Semnopitheci;* and although its tail is very long, it is not particularly thin. Major Sykes referred it provisionally to the *Semnopitheci,* until by an examination of its posterior molars its real station in the system should be determined.

It is thus characterized:

SEMN.? ALBOGULARIS, Sykes. *Semn.? suprà flavo nigroque, infrà albo nigroque irroratus; gulâ albâ; artubus nigris: mystacibus latis aures penè obvelantibus; superciliorum pilis rigidis exstantibus.*

Hab. in Madagascar?

Its canines are remarkably long (nearly $\frac{3}{4}$ of an inch), slender, sharp; the incisors very short and even. Head rounded and short. Ears very small, nearly rounded, and for the most part concealed in the long hair about the head. Eyes deeply seated, and shaded by a continuous arch of long hairs directed forwards. *Irides* broad; of a brown ochre colour. Hair forming a bunch on each cheek and resembling whiskers: no beard. Cheek pouches rudimentary only, not observable externally, even when filled, being concealed by the bushy hair of the cheeks. Thumbs of anterior hands short and distant; those of the posterior long. Whole of the upper surface of the animal of a mingled black and yellowish ochre colour, each hair being banded black and ochre; the black prevailing on the shoulders, the ochre on the back and flanks. Under surface grizzled white and black. Anterior limbs uniform black; posterior black with a little of the dorsal colour. Chin and throat pure white. Tail black, half as long again as the body.

The manners of this monkey are grave and sedate. Its disposition is gentle but not affectionate: free from that capricious petulance and mischievous irascibility characteristic of so many of the African species, but yet resenting irritating treatment, and evincing its resentment by very smart blows with its anterior hands. It never bit any person on board ship, but so seriously lacerated three monkeys, its fellow passengers, that two of them died from the wounds. It readily ate meat, and would choose to pick a bone, even when plentifully supplied with vegetables and dried fruits.

Mr. Gray exhibited a specimen of a *Tortoise* which he regarded as the type of a new genus in the family *Emydidæ.* It is characterized as follows:

PLATYSTERNON.

Sternum latum, anticè truncatum, posticè emarginatum. Scutella sterni

sterni 12: quorum duo anteriora brevia, lata, per totam sterni latitudinem extensa. Symphysis scutellorum pectoralium abdominaliumque extremitatibus tecta: scutellis axillari inguinalique magnis; inter quæ scutellum tertium accessorium iis simile; scutella hæc tria in suturam symphysis inserta.

Caput maximum, cute corneâ continuâ tectum. Cauda longissima, teres, attenuata; supernè serie unicâ, infernè duplici, squamarum tecta; haud cristata.

This genus is intermediate between *Emys* and *Chelydra*. It has the broad *sternum* and simple tail of the former genus; and possesses, in common with the latter, a large head, and the peculiar plates which are situated between the outer extremities of the pectoral and abdominal, and the marginal dorsal plates. It differs from *Chelydra*, however, in the peculiar plate which covers the *symphysis* of the *sternum* being here comparatively very small, not exceeding in size the axillary and inguinal plates, and in its being inserted in the same line with them.

The only species known was characterized as the

Platysternon megacephalum. *Plat. capite brunneo, obscurè nigro radiato: testâ supernè saturatè brunneâ, infrà pallidè flavâ: marginibus scutellorum sulcis aliquot obscuris striisque radiantibus confertis.*

Long. testæ, $3\frac{1}{2}$ unc.; sterni, $2\frac{3}{4}$: latitudo testæ, $2\frac{5}{8}$; sterni anticè, $2\frac{1}{4}$: long. capitis $2\frac{5}{8}$; caudæ, 3.

Hab. in Chinâ.

In illustration of the conterminous genus *Emys*, Mr. Gray exhibited a specimen of the *Em. Caspica*, Schw., recently obtained from the Mediterranean.

Mr. Gray also exhibited a specimen of the animal (*Ocythoë*) found in the shells of the genus *Argonauta*, in illustration of some observations on the disputed question of its parasitic or non-parasitic nature. He stated that he had lately examined ten specimens, four of them referable to *Ocythoë Cranchii*, and the remainder to *Ocythoë antiquorum*; there being, however, little to distinguish them except the size. All these specimens, as well as all those which have been figured, were females, and had eggs inclosed in the hinder part of the shell, in the cavity which is uniformly found behind the body of the animal. In all, the posterior *siphon* was placed more or less exactly in the keel of the shell, but the body did not always occupy a symmetrical position with regard to it, the eye of one side being sometimes nearer to the spire than that of the opposite side. Only one or two of these individuals had their bodies marked with the ridges of the shells, the impressions of which were, however, mostly observable upon the arms. The animals all appeared to be retained in the shells by the inflection of the anterior pair of arms. Mr. Gray added that he had also lately seen several specimens preserved without shells, and having their bodies shaped exactly like that of the common *Octopus*, without the slightest appearance of their having been inclosed in shells: the history of these specimens

he

he was unable to trace, and he could not therefore affirm that they were found in the state in which he observed them.

From these facts Mr. Gray stated that he was inclined to regard it as probable that the *Ocythoë* is only parasitic in the shell of *Argonauta;* that the shells are only resorted to by females during the breeding season for the protection of their eggs; and that the chief purpose of the dilated portion of the anterior arms is to retain the animal in the shell. He remarked, that no author, so far as he was aware, had distinctly stated of his own observation that these parts are expanded in the form of sails before the wind, a service which they seem to be incapable of performing, except in poetic fiction.

XL. *Intelligence and Miscellaneous Articles.*

RED COLOURING MATTER PRODUCED BY THE ACTION OF NITRIC ACID UPON ALCOHOL, &c. BY M. ROUCHAS.

WHEN equal quantities of nitric acid and alcohol act upon each other, the products are, azote, nitrous and nitric oxide, carbonic, acetic and nitrous acid, water, and nitric æther: if one part of alcohol and three parts of acid be used, oxalic acid is obtained. M. Rouchas remarks, that having lately caused three parts of nitric acid to act upon one part of alcohol, the above-mentioned products not only resulted, but also that when ammonia, potash, or soda, or their carbonates or bicarbonates, were added to the solution, a fine red colour was produced, which he considers as a new fact. The same effects are produced by sugar, starch, and some other vegetable substances.

M. Rouchas observes that this colour does not appear to be similar to that which accompanies purpuric acid; and he concludes from his experiments:

1st, That nitric acid, while acting on alcohol, sugar, starch, &c. among other well-known products, occasions the formation of a peculiar non-azotized red colour.

2ndly, That the alkali acts in developing the colour merely by neutralizing the excess of nitric acid which exists in the solution, and thus separating it; for a fresh quantity of nitric acid has the property of causing the red colour to disappear, and it may be reproduced by a fresh addition of an alkaline substance.

3rdly, That this red principle is chemically composed of the same elements as sugar, alcohol, starch, &c., the hydrogen being in smaller quantity.

4thly, That the red colour developed on mixing sugar or gums in solution with nitrate of silver, arsenic acid with sugar, chlorine or bromine with sugar, is identical with the red principle obtained when nitric acid acts upon sugar, alcohol, &c.

5thly, That nitrate of silver, nitric acid, arsenic acid, chlorine and bromine, act upon the vegetable substances submitted to their agency, the first three by dehydrogenating them by means of their

oxygen, and the last two by combining with their hydrogen, on account of the great affinity for it to form hydracids.—*Journal de Pharmacie*, March 1831.

PERCHLORIC ACID.

M. Sérullas has discovered that perchloric acid is obtained by distilling chloric acid. The watery part is to be rejected as useless, and a colourless dense liquid is observed to adhere to the sides of the vessel. When the heat is increased, and which ought to be sufficiently strong to heat every part of the body of the retort, the liquid passes into the receiver; this is perchloric acid: although concentrated, it does not inflame paper like chloric acid, but it gives to the paper, when put in contact with a red-hot coal, the property of forming very vivid sparks.

During the distillation of the chloric acid, chlorine and oxygen separate, and a portion of the latter combining with the undecomposed chloric acid, it passes to the state of perchloric acid, which is very permanent, and may be distilled at a high temperature without any decomposition. The acid distilled has a light rose colour, probably derived from a little manganesiate of potash contained in the chlorate of potash; but on concentrating it by heat, it becomes colourless. To be quite sure that the chloric acid is pure, it may be redistilled. This process is much preferable to that of Count Stadion, which is complicated, and also dangerous in the execution. There remained some doubts as to the composition of perchloric acid. M. Sérullas has ascertained that it is composed of two atoms of chlorine and seven atoms of oxygen.—*Ibid.* p. 142.

RED SOLUTIONS OF MANGANESE.

The red colour of the sulphate of manganese procured under certain circumstances, has been attributed to the presence of peroxide, deutoxide and red oxide. Mr. Pearsall is, however, of opinion that it is caused by manganesic acid. Among other reasons which he assigns for this opinion, are the following: the red solutions of manganese and the solution of manganesic acid are both alike in colour and in bleaching power; both become colourless by the same agents, and lose their bleaching power by losing their coloured state; they are similarly affected by reagents, and afford similar red crystallized salts: when deprived of colour they afford a crystallized colourless proto-salt, and both are compatible with certain other solutions of other substances.—*Journal of the Royal Institution*, Aug. p. 62.

POWERFUL ELECTRO-MAGNET.

Professor Henry and Dr. TenEyck have constructed an electro-magnet for Yale College, which is stated to have sustained 2063 pounds. The magnet is wound with 26 strands of copper bell-wire, covered with cotton thread, 31 feet long; about 18 inches of the ends are left projecting, so that only 28 feet of each actually surround the iron; the aggregate length of the coils is therefore

728 feet.

728 feet. Each strand is wound on a little less than an inch; in the middle of the horse-shoe it forms three thicknesses of wire, and on the ends or near the poles it is wound so as to form six thicknesses. With a battery of 4⅞ square feet, the magnet suspended 2063 pounds. The effects of a larger battery were not tried. It induced magnetism in a piece of soft iron so energetically as to raise 155 pounds. When two batteries were employed so that the poles could be rapidly reversed, a curious fact was observed. After one of the batteries had been removed, the curvature, with a weight added, in all 89 pounds, remained suspended and did not fall when the poles were reversed. This effect must have been instantaneous, otherwise the weight must have fallen; as there was an instant when the magnet could have had no power. It was attempted to decompose water by this magnet, but without success.—*Ibid. Silliman's Journal.*

MARKING-INK FOR LINEN.

M. Henry, senior, recommends the following as a marking-ink for linen to be employed in hospitals.

Take of iron filings........................... 1 pound.
Acetic acid (*Vinaigre de Bois*) sp. gr. about 1·052.. 2 pounds.

Mix the iron filings with half the vinegar; shake the mixture frequently, and as it becomes thick, add the rest of the acetic acid, and of water.................................. 1 pound.

Heat the mixture to favour the action of the acid upon the iron; and when it is dissolved, add

Sulphate of iron............................ 3 pounds.
Gum arabic................................. 1 pound.
Previously dissolved in water...................... 4 pounds.

Mix them thoroughly while hot; these quantities usually give 12 pounds of product. In order to employ it, the linen is stretched upon a table, and copper characters [stencils?] and a hair-brush are used.—*Journal de Pharmacie*, July 1831.

MONTHLY AMERICAN JOURNAL OF GEOLOGY AND NATURAL SCIENCE.

We have lately received the first Number (for July) of this work, which is published at Philadelphia, and conducted by G. W. Featherstonhaugh, Esq., F.G.S., &c. &c. Of the design, as expressed in the Prospectus, which contains some candid remarks on the state of the cultivation of natural history in the United States, and on the means necessary for its improvement, we highly approve. Of the execution also, so far as the Number before us will enable us to form a judgement, we are happy to express our approbation. Desirous of seeing our Transatlantic brethren the emulous yet generous rivals of the cultivators of science in Britain, in every department of human knowledge, Mr. Featherstonhaugh's Journal has our most cordial wishes for its success.

The Number begins with the "Prospectus," which is followed by an "Introduction," giving some excellent remarks on the Mosaic history of the Creation, as fully reconcileable with the inferences of geology,

geology, but leaving geologists to the free exercise of induction from observed facts, in ascertaining the circumstances and phænomena under which that work of Omnipotence was effected. This is succeeded by a notice by the Editor (illustrated by a good lithograph) of *Rhinoceroides Alleghaniensis*, a new fossil genus of *Pachydermata*, a portion of a jaw of which has been discovered in the diluvium or alluvium of Pennsylvania, agreeing in most of its proportionate dimensions with that of *Rhinoceros*, but differing from it by "the great space between the intermaxillary suture (very distinct in the fossil,) and the place of the first molar, being in the fossil twice as much as in the recent *R. Indicus*;" and also by "the occupation of two incisors in the fossil, of the space allotted to one incisor in the *R. Indicus*."

The next article, which is also by the Editor, is "On the Ancient Drainage of North America, and the Origin of the Cataract of Niagara," illustrated by a lithographic "flat view" of that cataract. In a note to this paper, we have the following remark on a subject not long since discussed in our pages: "That the recession of these falls is effected as Mr. Lyell supposes, we have never doubted; but a long and familiar acquaintance with the cataract has induced us to adopt the opinion we have just seen announced by the Rev. W. D. Conybeare (Phil. Mag. and Annals, No. 52, April 1831, page 267), that in forming the first estimates of this [Mr. Lyell's] computation [that Lake Erie will be reached in 30,000 years] 'some partial degradation of the strata has been here mistaken for the general retrogradation.'" We then have "The Diary of a Naturalist," kept at the Bartram Botanic Garden, near Philadelphia. Some observations on the vagaries of nomenclature recently exhibited by certain French geologists, compose the next article. The remainder of the Number consists of some excellent remarks on the adjudication of the first Wollaston prize to Mr. Smith, animadverting with much justice on Dr. Brewster's treatment of the Geological Society on that account; an account of Lord Bridgewater's bequest, from the Phil. Mag. and Annals for March last; an essay by a correspondent on the "Influence of Climate on the Fruitfulness of Plants," and various scientific memoranda, including a notice of some newly discovered remains of the Mastodon, among which is a skull in better preservation than any yet discovered of this animal.

Again we express our cordial wishes for the prosperity of this work, which we are convinced will perform very important services to science.

September 28, 1831.

MR. HARVEY'S RESEARCHES ON NAVAL ARCHITECTURE.

Mr. Harvey of Plymouth has received from the Emperor of Russia, a splendid diamond ring, on account of his recent researches on ship-building. A very flattering letter accompanied the present.

ALTERA-

ALTERATIONS AND ERRATA

In Mr. Waterston's "Exposition of a New Dynamico-Chemical Principle," inserted in the Phil. Mag. and Annals for September.*

In page 175 *leave out* the words following the formula $\frac{1\cdot5708\,cp}{\cdot5236\,df}$ beginning with "*if no elastic force is supposed to be exerted. Since perfect resiliency however*" &c., and ending with "*further to diminish the rectilineal motion of the particles.*"

Insert in place of the above.—If we suppose it equally probable that the point of concurrence *p*, may be anywhere situated in the lines *ab*, *df*, the extreme cases will be where it coincides with their extremities and centres of gravity. In the former by the above formula the rotatory momentum would be $\frac{3}{5}$ of the whole, in the latter it is 0. The mean quantity or $\frac{3}{10}$ will therefore be the ratio of the whole quantity of rotatory momentum generated in the medium on this supposition. But amidst the diversified concourse of the particles, there are peculiar modes of concurrence which do not hold with this supposition and which in regard to the total effect throughout the medium will tend to augment the above ratio.

Page 170, line 23, for *actual* read *active.*
— 174, — 31, — *particulars* — *postulates.*
— 177, — 28, — *will continually replace the,* read *will be continually renewed by the*
— 179, — 34, — *theories* read *theorems.*

LIST OF NEW PATENTS.

To W. Sumner, Hose, Leicestershire, lace-maker, for certain improvements in machinery for making lace, commonly called bobbin net.—Dated the 3rd of February, 1831.—6 months allowed to enrol specification.

To G. G. Gardner, New York, but now residing at Threadneedle-street, gentleman, for an improved roving machine. Communicated by a foreigner.—11th of February.—6 months.

To W. W. Richards, Birmingham, gun-maker, for certain improvements in the touch-holes and primers, suitable to percussion guns, pistols, and all sorts of fire-arms fired upon that principle.—11th of February.—2 months.

To J. Gunby, George-street Sand Pits, Birmingham, artist, for an improved method or methods of combining glass with metal, metals, or other substances, applicable to various useful and ornamental purposes.—11th of February.—2 months.

To C. Guillotte, Crispin-street, Spitalfields, machine-maker, for an improvement in the rack applicable to the battons of looms, or machinery for weaving plain or figured ribbons. Partly communicated by a foreigner.—11th of February.—6 months.

* Communicated by Mr. Waterston.—We received this article last month, but too late to allow of its insertion in its proper place.

LUNAR

LUNAR OCCULTATIONS FOR OCTOBER.

Occultations of Planets and fixed Stars by the Moon, in October 1831. Computed for Greenwich, by Thomas Henderson, *Esq.; and circulated by the Astronomical Society.*

1831.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean solar time.	Immersions. Angle from North Pole.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean solar time.	Emersions. Angle from North Pole.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Oct. 21	ξ^2 Ceti	5	255	22 33	8 35	80	44	23 27	9 28	328	297
	μ Ceti......	4	293	7 30	17 30	89	128	8 26	18 26	303	343
22	*f* Tauri ...	5·6	379	0 49	10 47	68	37	1 39	11 36	334	311
23	γ Tauri ...	3·4	478	Under	horizon	...	...	20 59	6 54	274	237
	θ^1 Tauri ...	5	510	23 35	9 28	73	33	0 24	10 18	321	283
	θ^2 Tauri ...	5·6	511	23 46	9 40	42	2	0 12	10 5	352	313
	75 Tauri...	6	508	23 51	9 45	165	124	0 24	10 17	229	190
	(99) Tauri	5·6	516	0 20	10 13	93	54	1 21	11 15	301	267
	Aldebaran	1	528	3 5	12 59	84	63	4 12	14 5	308	305
24	115 Tauri	5·6	651	22 33	8 23	76	38	23 19	9 9	308	268
	119 Tauri	5·6	663	1 8	10 57	192	152	☽ touching Star. Occulted to places further South.			
	120 Tauri	6	667	1 15	11 5	139	99	2 6	11 55	245	207
26	*g* Geminor.	6	951	5 57	15 38	100	74	7 10	16 51	250	243
30	σ Leonis...	4	1334	4 52	14 18	38	359	5 40	15 6	288	249

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1831.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·296. Aug. 22. Wind N.E.—Min. 29·655. Aug. 7. Wind S.E.
Range of the mercury 0·641.
Mean barometrical pressure for the month .... 29·986
Spaces described by the rising and falling of the mercury............ 3·559
Greatest variation in 24 hours 0·317.—Number of changes 16.
Therm. Max. 77°. Aug. 1. Wind N.E.—Min. 52°. Aug. 17. Wind N.
Range 25°.—Mean temp. of exter. air 65°·13. For 31 days with ☉ in ♌ 65·43
Max. var. in 24 hours 22°·00.—Mean temp. of spring-water at 8 A.M. 52·98

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 9th...... 92°
Greatest dryness of the atmosphere, in the afternoon of the 1st...... 44·0
Range of the index .... 48·0
Mean at 2 P.M. 55°·5.—Mean at 8 A.M. 64°4.— Mean at 8 P.M. 70·2
—— of three observations each day at 8, 2, and 8 o'clock .... 63·3
Evaporation for the month 3·35 inches.
Rain in the pluviameter near the ground 1·815 inch.
Prevailing wind, West.

Summary of the Weather.

A clear sky, 2; fine, with various modifications of clouds, 21; an overcast sky without rain, 5; rain, 3.—Total 31 days.

Clouds.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
24	19	30	0	26	20	14

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3½	2	3	2½	1	5½	7½	6	31

General Observations.—This month has been remarkably fine, dry, and warm, so that very little interruption was experienced in getting in the full crops of corn, which were carried in good condition in this neighbourhood. The only heavy and continued rain was on the 2nd instant, when upwards of an inch fell in seven hours, accompanied with lightning and thunder, and crossing winds; but on this occasion there was scarcely any difference in the pressure of the atmosphere. In the evenings of the 10th and 11th twelve meteors appeared; many of them were large, with long sparkling trains. On the 17th lightning and thunder again occurred, and, as nearly opposite winds prevailed at the same time, a smart shower of rain fell here; for opposite winds, which of course have different temperatures, invariably condense the atmosphere they pervade, and induce rain. Some injury was done by the lightning in Hampshire. On the 18th, 19th, 20th and 21st, strong gales of wind blew, first from the West, then from the North.

The mean temperature of this month is about one and a half degree higher than the mean of August for many years past, which was occasioned by the comparative dryness of the weather, and the consequent strong terrestrial radiation.

The atmospheric and meteoric phænomena that have come within our observations this month, are, two solar halos; seventeen meteors; lightning and thunder on two days; and four gales of wind, two from the North, and two from the West.

REMARKS.

London.—August 1. Fine: rain at night. 2. Heavy showers, with thunder. 3. Rain. 4. Cloudy: lightning at night. 5. Warm, with heavy thunder-showers. 6—8. Very fine. 9. Fine: rain. 10—15. Fine. 16. Slight fog: sultry: lightning at night. 17. Fine in the morning, with showers at intervals: a very heavy thunder-storm in the afternoon, with rain in torrents, mixed with hail; the latter however was so nearly melted that it did but little damage. 18. Fine. 19, 20. Windy: rain at nights. 21. Showers: fine. 22—29. Very fine. 30. Fine: cloudy at night, and windy. 31. Rain: fine, but cool at night.

Penzance.—August 1—3. Clear. 4, 5. Fair. 6. Heavy showers. 7, 8. Fair. 9. Showers. 10—12. Clear. 13—16. Fair. 17. Clear. 18. Fair: showers. 19. Showers. 20—23. Clear. 24. Rain. 25. Fair. 26. Fair: rain. 27. Misty. 28, 29. Clear. 30. Fair. 31. Misty: rain.

This month has been unusually dry; the quantity of rain fallen is 1·8150 of an inch, which is less by 0·3000 of an inch than has fallen in the month of August during the last ten years, and 2·0757 below the average of that period.

Boston.—August 1. Cloudy: rain at night. 2—4. Cloudy. 5. Rain: thunder and lightning A.M.: rain all day. 6. Foggy. 7. Fine: rain, with thunder and lightning P.M. 8. Cloudy. 9. Cloudy: rain at night. 10, 11. Fine. 12. Cloudy. 13, 15. Fine. 16. Cloudy: heavy rain, with thunder and lightning. 17. Cloudy: rain P.M. 18. Fine. 19. Stormy: rain early A.M. 20. Stormy: rain P.M. 21. Stormy. 22, 23. Fine. 24. Fine: rain P.M. 25, 26. Fine. 27. Cloudy. 28—30. Fine. 31. Cloudy.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1831.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
☾ Aug. 1	30·034	29·938	29·95	29·95	30·074	29·986	29·44	77	61	75	61	77	62	64	E.	E.	NE.	NE.	...	0·03	...	0·040	...
2	29·879	29·854	29·95	29·95	29·905	29·877	29·27	75	60	74	60	74	61	65	NE.	NE.	NW.	NE.	...	·43	...	1·030	0·37
3	29·872	29·860	29·95	29·92	29·877	29·856	29·30	78	65	73	60	76	61	66·5	E.	NE.	E.	NE.	0·35	·05	...	...	...
4	29·782	29·656	29·80	29·80	29·790	29·709	29·17	81	61	72	61	74	61	67	S.	N.	SE.	calm	...	·01	...	...	...
5	29·668	29·654	29·80	29·80	29·731	29·693	29·04	82	57	72	60	74	60	64	W.	NW.	W.	calm	...	·03	...	·010	·39
6	29·721	29·695	29·70	29·60	29·745	29·701	29·06	78	56	71	59	74	62	64·5	S.	SW.	SW.	calm	·30	...	0·875	...	1·69
● 7	29·729	29·642	29·80	29·60	29·699	29·655	29·05	82	55	67	56	73	58	68	S.	N.	SE.	SE.	...	...	...	...	...
8	29·927	29·830	30·00	29·95	29·930	29·815	29·20	83	57	66	57	73	56	68	N.	NW.	NW.	E.	...	...	...	...	·64
9	29·935	29·873	29·92	29·90	29·978	29·954	29·26	84	58	68	57	73	60	65	W.	NW.	SW.	W.	·25	·21	·200	·220	...
10	30·130	30·024	30·05	30·00	30·140	30·050	29·37	75	50	67	54	70	55	63	W.	NW.	NW.	NW.	...	...	...	...	·40
11	30·150	30·127	30·10	30·10	30·152	30·136	29·55	78	50	71	54	72	58	66	E.	NE.	SE.	NW.	...	...	...	...	...
12	30·138	30·074	30·10	30·10	30·152	30·105	29·50	81	51	72	53	72	57	64	W.	NE.	SE.	NW.	·50	...	...	...	...
13	30·050	29·994	30·00	29·98	30·070	30·007	29·43	80	51	69	58	75	61	64·5	W.	NE.	E.	NE.	...	...	...	·020	...
14	30·037	29·989	29·98	29·95	30·040	29·994	29·43	76	47	72	59	73	57	63	W.	NE.	NW.	NE.	...	...	...	...	...
☽ 15	30·106	30·081	30·00	30·00	30·125	30·078	29·50	77	51	73	61	70	60	63	E.	SW.	S.	E.	·30	...	...	...	...
16	30·125	30·008	30·02	30·02	30·122	30·101	29·54	75	55	73	60	74	59	62·5	SW.	SE.	W.	calm	...	·03	...	...	...
17	30·052	29·960	30·04	30·02	30·077	30·028	29·44	79	52	70	60	73	52	62	N.	N.	N.	calm	...	·54	...	·280	1·05
18	29·959	29·934	30·05	30·00	30·019	30·002	29·37	72	53	65	55	66	57	55·5	N.	NW.	NW.	N.	·30	·06	...	·010	·07
19	29·698	29·677	29·90	29·90	29·797	29·767	29·03	70	54	67	56	70	57	64	NW.	NW.	W.	NW.	...	·03	·120	·010	·07
20	29·936	29·668	30·00	29·95	29·965	29·763	29·20	72	57	67	56	69	58	58·5	W.	NE.	N.	NE.	...	·14	...	·060	...
21	30·258	30·087	30·15	30·00	30·229	30·080	29·56	70	52	67	56	69	54	63	N.	NE.	N.	NE.	·35	...	...	...	·24
22	30·293	30·225	30·20	30·20	30·296	30·252	29·73	72	54	68	54	69	54	60	NE.	N.	NE.	NW.	...	...	...	...	...
○ 23	30·175	30·004	30·15	30·10	30·192	30·119	29·58	81	49	68	54	76	57	64	W.	NE.	W.	NE.	...	...	...	...	...
24	29·949	29·785	29·90	29·80	30·002	29·808	29·27	73	56	67	56	72	58	61·5	S.	SW.	SW.	W.	·30	·01	·500	·085	...
25	29·881	29·722	29·88	29·80	29·903	29·783	29·02	75	45	67	54	69	56	60	W.	NW.	W.	NW.	...	...	...	...	·11
26	29·958	29·936	29·88	29·88	29·980	29·964	29·30	74	57	71	54	69	60	61	S.	W.	SW.	NW.	...	...	·060	...	...
27	29·949	29·932	29·90	29·88	29·967	29·962	29·30	78	51	70	62	74	55	64	S.	SW.	SW.	W.	·30	...	...	...	...
28	30·175	30·105	30·12	30·00	30·189	30·099	29·43	74	51	67	54	71	56	61·5	W.	NW.	W.	W.	...	...	...	...	...
29	30·214	30·175	30·12	30·12	30·242	30·231	29·56	77	56	67	56	69	60	63	SW.	W.	W.	W.	...	...	...	...	...
☾ 30	30·124	30·012	30·12	30·10	30·166	30·092	29·39	75	62	68	57	73	64	67	SW.	W.	SW.	W.	...	...	...	...	...
31	29·984	29·937	30·00	30·00	29·997	29·957	29·26	71	46	67	63	74	55	61·5	NW.	W.	W.	NW.	·40	·02	·060	·050	...
	30·258	29·642	30·20	29·60	30·296	29·655	29·34	84	46	75	53	77	52	63·3					3·35	1·59	1·815	1·815	5·03

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XLI. *On Vanadium.* *By* M. Berzelius*.

VANADIUM was discovered in the year 1830 by Sefström, in a Swedish iron, remarkable for its ductility, obtained from the iron mine of Jaberg, not far from Jönköping in Sweden. The name of this metal is derived from that of *Vanadis*, a Scandinavian divinity. It is not yet known under what form, or in what state of combination, vanadium occurs in the ore of Jaberg. It is also found in Mexico, in a lead mine at Zimapan. Del Rio, who analysed this mineral in 1801, announced the discovery of a new metal in it, which he called *Erythronium;* but the same mineral having soon afterwards been analysed by Collet Descotils, he asserted that erythronium was merely impure chromium. Del Rio himself adopted the opinion of the French chemist, and considered the mineral as a subchromate of lead; thus the metal, so near being discovered, remained thirty years unknown to chemists. Since the discovery of vanadium by Sefström, Wohler has ascertained that the mineral of Zimapan contains vanadic and not chromic acid.

I have had an opportunity of studying the properties of this metal, and those of its combinations, by means of specimens presented to me by M. Sefström for that purpose.

M. Sefström having ascertained that the finery cinder of the cast-iron of Jaberg contained more vanadium than the iron itself, made use of it to obtain the metal, which occurs in it in the state of vanadic acid. For this purpose he uses the following process: the finery cinder is powdered and mixed with

* *Traité de Chimie*, tom. iv. p. 642. See also the present volume of Phil. Mag. and Annals, p. 151, 157, and 209; and the *Miscellaneous Articles* in the present Number.

 nitre

nitre and carbonate of soda, in the proportions of one part of cinder, one of nitre and two parts of carbonate; this mixture is strongly calcined for an hour. The soluble portion of the powdered mass is dissolved by boiling water, the solution is filtered, and the excess of alkali saturated with nitric acid and afterwards precipitated with muriate of barytes or acetate of lead. The precipitate is vanadate of barytes or lead, containing also some phosphate of barytes or lead, silica, zircon, and alumina. Whilst it is still moist, it is to be decomposed by concentrated sulphuric acid; the solution immediately becomes of a deep red colour, and after having digested the mixture for half an hour, alcohol is added to it, and it is again digested; æther is then formed, and the vanadic acid is reduced to the state of salifiable oxide, the solution of which is blue; and when it begins to assume a syrupy consistence, it is mixed in a platina crucible with a little fluoric acid to separate a portion of silica, which it is almost impossible to get rid of in any other manner; the evaporation is continued over the naked fire, and the sulphuric acid is at last expelled at a red heat. The residue is impure vanadic acid. It is fused with nitre, added in small portions at a time. The vanadic acid combines with the potash and expels the nitric acid, and nitre is added until it is found that on cooling a small portion of the mass it ceases to be red. The alkaline carbonates may be employed; but when nitre is used, the zircon and alumina remain less acted upon, when the vanadate of potash is dissolved. The mass is afterwards dissolved in water, and after filtration the residue is slightly washed; it still contains vanadium, and ought not to be thrown away. A piece of sal ammoniac, larger than can be dissolved by it, is to be put into the filtered liquor. As this salt dissolves, a white pulverulent precipitate is formed, which is vanadate of ammonia, insoluble in a saturated solution of sal ammoniac. The phosphate of ammonia remains dissolved; but when the solution is alkaline, which happens when a carbonate is employed to dissolve the vanadic acid, insoluble subphosphate of ammonia always precipitates. The vanadate of ammonia ought to be washed, first with a solution of sal ammoniac, and afterwards, to remove the sal ammoniac, with alcohol of 0·86. It is to be again dissolved in boiling water, mixed with a little ammonia, filtered and left to crystallize. It is from this salt that vanadic acid and oxide are afterwards obtained, by heating it gently in open vessels to procure the former, and in closed vessels to prepare the latter.

The residue which has been mentioned is a compound of vanadic acid, alumina, zirconia and silica. The vanadium is extracted

extracted from it by means of an alkaline hydrosulphuret, by fusing the residue with sulphur and carbonate of potash. Sulphovanadate of potash is formed, from which sulphuret of vanadium may be precipitated by sulphuric acid.

Vanadium is very difficult of reduction by the usual methods, that is to say, by heating the oxide in a charcoal crucible; for it is reduced only at the places in which it is in immediate contact with the charcoal, and the interior is a suboxide, as infusible as the metal itself at the temperature at which manganese undergoes fusion.

With potassium the reduction is easy; pieces of vanadic acid, which have been previously fused, are to be mixed with pieces of potassium of equal bulk in a porcelain crucible; the cover is to be well fastened on, and the crucible is to be heated with a spirit-lamp. The reduction occurs almost instantaneously with a kind of detonation. The crucible, when cold, is to be put into water to dissolve the potash, and the reduced vanadium is to be collected on a filter; it is obtained in the state of a black powder, which shines in the sun, and takes a grayish metallic lustre underthe burnisher. But in this way a true idea of the aspect of the metal is no more obtained, than of that of gold precipitated from solution by the salts of iron.

When the method discovered by M. Rose for the reduction of titanium is employed to reduce vanadium, the experiment succeeds more completely than with potassium. For this purpose, chloride of vanadium is prepared, by passing a current of dry chlorine over a mixture of vanadic acid and very dry charcoal. This chloride is a volatile fuming fluid, and it is to be introduced into a glass bulb blown on a barometer tube; a current of dry ammoniacal gas is passed through the tube until the chloride is entirely saturated. Sal ammoniac sublimes, which may be expelled from the tube by another spirit-lamp. The reduced vanadium remains in the bulb, and an inconsiderable portion is reduced at that part of the tube which is kept hot. On cutting the bulb afterwards in two, the vanadium is found in the state of a silvery white stratum, which on the side next the glass reflects like a mirror, and is white like polished steel. If water and atmospheric air are not entirely excluded, a small quantity of black powder is found in the middle of the mass; this is suboxide of vanadium, and is easily detached.

Vanadium is white, and when its surface is polished it resembles silver considerably, or molybdenum, which of all metals it is most like. It is not ductile, and is easily reduced to a powder of an iron-gray colour. I have not enough of it, nor are my specimens in a convenient form, to determine its

 specific

specific gravity. It is a good conductor of electricity, and strongly negative to zinc. The powder of vanadium, obtained by its reduction with potassium, takes fire at a heat below redness, burns without energy, and leaves a black unfused oxide. Vanadium dissolves readily in nitric acid and in aqua regia; the solution has a fine blue colour. The sulphuric, muriatic, and fluoric acids do not attack it at all, even when they are concentrated and boiling. It is not oxidized by the alkaline hydrates, and it may be heated with them to redness without undergoing any alteration if the air be excluded. The solution of vanadic oxide in the acids, or of vanadic acid in an excess of caustic potash, does not give metallic vanadium by zinc.

Oxides of Vanadium.—Of this metal there are three compounds with oxygen:—

1st, *Suboxide of Vanadium.*—It is obtained by reducing vanadic acid by hydrogen gas at a red heat, or by fusing vanadic acid in a cavity made in charcoal. In the first mode, the suboxide preserves the form and lustre of the crystalline facets of the acid, but it becomes black; by the latter process a coherent mass is obtained, which is easily reducible to powder, possesses a semimetallic lustre and the colour of plumbago. Hydrogen passed over the suboxide does not decompose it at the highest temperature which can be imparted to it in a porcelain tube heated by a small wind-furnace. This suboxide, by whatever process obtained, provided it be coherent, is a good conductor of electricity, and infinitely exceeds copper, and even gold and platina, as a negative electromotor.

It has not hitherto been combined with other bodies, or with acids or bases. That which is reduced by hydrogen gas gradually oxidizes in the air, but without any alteration of appearance; and the lower the temperature at which the oxide is formed, the more readily oxidation occurs. Its oxidation is apparent by throwing it into water, which becomes of a fine green colour by dissolving a compound presently to be treated of. When heated in the air, it takes fire and burns, leaving an unfused black residue. Chlorine gas converts it into chloride and vanadic acid. It is composed of 89·538 parts of vanadium, and 10·862 parts of oxygen; 100 parts of the former are combined with 11·6843 parts of the latter.

2ndly, *Oxide of Vanadium.*—Vanadate of ammonia cannot be employed in the preparation of this oxide in the same way as the molybdate and tungstate of ammonia, which yield the oxides of their metals when they are heated. The oxygenated compound of vanadium obtained by this method, contains the three degrees of oxidation of this metal. In order to obtain pure

pure oxide of vanadium in the dry way, 9·5 parts of suboxide of vanadium are to be mixed with 11·5 parts of vanadic acid, and the mixture is to be heated to whiteness, in an atmosphere of carbonic acid gas. In the moist way, it may be obtained by precipitating a blue vanadic salt, previously treated by sulphuretted hydrogen, sugar or alcohol, in order to destroy all the vanadic acid which it may contain. This solution is to be precipitated by carbonate of soda, added slightly in excess. A grayish white precipitate is formed, which is collected on a filter and washed, without the contact of the air. It is to be pressed between folds of filtering paper and dried *in vacuo.* It is gray, inclining to brown: it is hydrated oxide of vanadium, sometimes containing traces of carbonic acid. When heated to redness *in vacuo*, it yields water and leaves the oxide in the state of a black powder, which does not blue litmus paper that has been previously reddened. Oxide of vanadium is not fusible at the temperature at which glass softens. It is insoluble in water, but if it remains long in it the water becomes gradually green, in consequence of increased oxidation. The hydrate rapidly oxidizes in the air, and becomes first brown and afterwards green; when dried it is black: it will be again noticed. Oxide of vanadium which has been heated, dissolves slowly but completely in acids; the solution is blue, and the oxide acts as a base; but it combines with bases and forms salts, which may be called *vanadites.* The alkaline carbonates dissolve it; the solution, which is of a deep brown, contains a vanadate and a bicarbonate; the bicarbonates also dissolve it, and assume a blue colour: it appears that this solution contains neutral double carbonate of vanadium and alkali. Oxide of vanadium is composed of 81·056 parts of vanadium, and 18·944 of oxygen, or 100 parts of the metal combine with 23·369 parts of oxygen, that is to say, with twice as much as in the suboxide.

3rdly, *Vanadic Acid.*—This is obtained by exposing vanadate of ammonia to a heat near redness in an open platina crucible, and stirring it occasionally. The vanadate decomposes, becomes at first black, and afterwards, in proportion as it absorbs atmospheric oxygen, of a red brown colour, which, by cooling, becomes gradually pale, and finishes by turning to a rust colour. The finer the sal ammoniac is powdered, the paler is the colour of the acid. The acid thus obtained, when triturated, becomes of the colour of the hydrate of iron, which forms on the surface of the metal immersed in water. It is tasteless and inodorous; it reddens the colour of moistened litmus paper. As soon as it is red hot it fuses. In this state it sustains a white heat without

losing

losing oxygen, if it be preserved from the influence of combustible bodies. When fused it crystallizes on cooling, and then exhibits a phænomenon, which merits observation. It solidifies at a heat which is invisible in daylight; but the moment that solidification commences, a luminous circle extends from the periphery to the centre, where, owing to latent heat becoming free, the mass remains red hot as long as the crystallization continues. The acid contracts much on solidifying, and is readily detached from the crucible: it is then of a yellowish red colour, and formed entirely of a mass of interlaced crystals. Cavities frequently occur containing small and perfectly regular crystals, the form and size of which may be determined, when opportunity offers of repeating the experiment with about 300 grains. Fused vanadic acid is translucent at the edges, and has a yellowish colour. When it is impure, or when it has been in part reduced to the state of oxide, it does not crystallize; but at the moment of its solidification, excrescences are produced in the form of cauliflowers, and the solidified mass is blackish. If the acid contains a very small quantity of oxide, it crystallizes, but afterwards assumes a violet colour. Vanadic acid is not a conductor of electricity. It is slightly soluble in water, to which it imparts a bright yellow colour. If the pulverulent acid be put into water and well stirred, it mixes with it so as to produce a turbid fluid of a yellow colour, which does not become clear for several days: 1000 parts of boiling water scarcely dissolve one part of vanadic acid, but the cooled solution remains transparent. The acid is deposited by evaporation in the form of red concentric rings. The last portion gives yellowish microscopic crystals, but they become green when heated. It is a compound of vanadic oxide and acid, produced apparently by the influence of dust floating in the air; a phænomenon similar to the partial reduction of a solution of oxymanganic acid, which is attributed to this cause. It is in general impossible to crystallize vanadic acid in the humid way, and it is equally so to extract it in an isolated state from a solution, because it combines equally with acids and bases. It is easily reduced to the state of oxide, especially under the influence of an acid; red nitric acid, sulphurous acid, several vegetable acids, especially the oxalic and tartaric, alcohol, sugar, &c. effect this reduction at a moderate temperature. Muriatic acid dissolves and becomes of an orange colour; but soon afterwards chlorine is disengaged, and the solution then possesses the property of dissolving gold and platina. Vanadic acid, fused on charcoal by the blowpipe, leaves a coherent mass, of the colour of plumbago, which is the suboxide of vanadium:

vanadium: with the phosphate of ammonia and soda it gives a fine green colour to glass, which appears brown while it is hot. The blue colour of the salts of vanadium cannot be produced, even on adding metallic tin to the flux. With borax it also gives a green glass. In this reaction vanadium resembles chromium, but the green colour produced by the former may be changed to yellow by the oxidating flame, which does not happen with chromium. This change is easily effected, especially with the glass of borax. With carbonate of soda it is not reduced to the metallic state. Vanadic acid is composed of 74·0449 parts of vanadium and 25·9551 of oxygen, that is to say, 35·0533 of the latter, and 100 of the former; consequently the metal is combined with three times as much oxygen as in the suboxide. Its saturating capacity is equal to one third of the quantity of oxygen which it contains, that is to say, 8·6517.

4thly, *Intermediate Oxides of Vanadium.*—We have seen that the suboxide and oxide of vanadium, when exposed to the influence of the air, acquire the property of colouring water green. The vanadic oxide and acid combine together in different proportions; two of these compounds have the property of forming with water a solution of a fine green colour. Other compounds are purple and orange coloured. They pass, by the influence of the air, from one degree of oxidation to a higher one.

a. Purple Oxide.— If vanadic acid be kept for twenty-four hours in a badly corked bottle, and water be then added to it, it becomes of a green colour. The mass is then to be poured upon a filter, and when the green liquor is filtered a fresh portion of water is added; the fluid which then filters is much deeper coloured and brownish; a fresh quantity of water assumes a fine purple colour, and when the washings have been thus continued for some time, the water passes through colourless. The residue exposed for some time to the air acquires the property of reproducing the phænomena which have been described, and eventually a new purple liquid is obtained. This solution holds but little matter in solution; it may be preserved in a full bottle hermetically sealed, but on exposure to the air it soon becomes green and afterwards yellow. The vanadic acid appears to be combined with the greatest quantity of oxide of vanadium that it is capable of rendering soluble. It may be called a subvanadate of vanadium.

b. Vanadate of Vanadium. — If hydrate of vanadium be allowed to dry in the air, and then digested in a very small quantity

quantity of water, it becomes of a green colour, which is beautiful, but so deep that the solution appeares opake. The solution, when filtered and evaporated *in vacuo*, leaves a blackish cracked residue, without any trace of crystallization, and which is completely resoluble in water. This same combination is obtained, when a solution of a neutral salt with a base of oxide of vanadium is mixed with neutral vanadate of potash. If the solutions are moderately concentrated, a great part of the new green compound formed is deposited in the state of deep coloured powder; and if the solution is too dilute to give a precipitate, one is obtained by dissolving sal ammoniac in it. The precipitate is insoluble in absolute alcohol, but it dissolves in alcohol of 0·86. The solutions of this substance diluted so as to become perfectly transparent, have a very fine green colour. A small quantity of alkali deepens the colour, but does not destroy the green compound. The addition of a caustic alkali in excess occasions in a short time a brown precipitate, which is a vanadate of the alkali added. The carbonates of soda and potash change the green colour to brown, without precipitating anything; an excess of carbonate of ammonia does not destroy the colour. Vanadic oxide, mixed and digested with vanadic acid, forms the same compound, which may also be produced in the dry way by heating an intimate mixture of $10\frac{1}{2}$ parts of oxide, and $23\frac{1}{10}$ parts of vanadic acid. The mixture fuses, and gives a glass of a deep green colour, the powder of which dissolves gradually in water.

c. Bivanadate of Vanadium.—This compound is obtained by mixing a neutral salt of vanadium with one of bivanadate of potash; this salt is solid and green like the preceding, the tint of which is deeper; its solution in water is of yellowish green. It is less soluble than the preceding, and is more completely precipitated by sal ammoniac.

d. Supervanadate of Vanadium.—All the purple and green compounds oxidize in the air, especially when they are very dilute. Their colour becomes first greenish yellow, and afterwards orange yellow. By spontaneous evaporation, they yield crystals of a pale orange yellow colour, which lose their water and become green when heated in the fire; 22·5 parts of water dissolve one part of this orange compound, consequently it is much more soluble than vanadic acid alone.

Sulphurets of Vanadium.—The affinity of vanadium for sulphur is but weak at moderately high temperatures; it may be mixed with sulphur, and the mixture may be distilled without undergoing combination; and even when it is heated to redness in an atmosphere of sulphur, vanadium is not sulphuretted.

retted. Nevertheless, there are several modes of obtaining sulphurets of vanadium; hitherto, only two have been formed, proportional to the oxide and vanadic acid.

1. *Sulphuret of Vanadium.*—This is obtained in the dry way by exposing suboxide of vanadium to a current of sulphuretted hydrogen at a red heat. Water, hydrogen, and even sulphur are disengaged, and the vanadium is slowly converted into a sulphuret. This sulphuret is black; it becomes compact by pressure; when burnished it has not a metallic lustre. Heated in platina foil, it burns with a blue flame, and leaves upon the platina a circular pellicle, which is translucent, blue at the circumference and purple nearer the sulphuret. Water does not remove this pellicle, but it disappears at a red heat, leaving minute drops of vanadic acid. In this state sulphuret of vanadium is entirely insoluble both in sulphuric and muriatic acids, and in the caustic alkalies. Nitric acid converts it into sulphate of vanadium.

The salts of vanadium are not decomposed by sulphuretted hydrogen, but the hydrate and the salts of vanadium are converted by the hydrosulphurets into sulpho-vanadates, which dissolve in water; the solution has a rich purple colour. Acids poured into these solutions occasion a brown precipitate, which soon subsides and then appears black; it is the sulphuret of vanadium: it may be washed and dried without undergoing any alteration; it dissolves with a purple colour in the alkaline hydro-sulphurets. The alkaline carbonates also dissolve it when boiling, but the colour of the solution is brownish yellow. The sulphuric and muriatic acids do not decompose it, although the liquid from which it has been precipitated retains a bluish tint, in consequence of the decomposition of a small quantity of nascent sulphuret. It is composed of 68·023 parts of vanadium, and 31·977 parts of sulphur.

2. *Supersulphuret of Vanadium.*—The affinity of sulphur for vanadium is so weak, that when a current of sulphuretted hydrogen is passed into an aqueous solution of vanadic acid, the precipitate is merely oxide of vanadium intimately mixed with sulphur, from which the acids separate the oxide without disengaging sulphuretted hydrogen, and leave the sulphur. To obtain the supersulphuret of vanadium, vanadic acid must be dissolved in an alkaline hydro-sulphuret, or the solution of a neutral vanadate of an alkali must be decomposed by sulphuretted hydrogen, and the sulphuret must be afterwards precipitated by sulphuric or muriatic acid. The colour of the precipitate is brown, but much less deep than that of the preceding sulphuret; and there is this peculiar circumstance attending it, that at the time of the addition of the acid it de-

composes a portion of the supersulphate of vanadium in its nascent state, and much more than of the sulphuret under the same circumstances. The supersulphuret of vanadium may be dried and kept without suffering any alteration; it appears to be black, but the powder is brown. At a high temperature it yields sulphur, and is converted into sulphuret: it dissolves in the same menstrua as the sulphuret, but its solutions have a deep colour resembling that of strong beer. Sulphuric and muriatic acid do not decompose it. It is composed of 58·647 parts of vanadium, and 41·353 of sulphur.

Phosphuret of Vanadium.—When vanadium is heated to redness in an atmosphere of phosphorus in vapour, they do not combine; but when phosphate of vanadium is heated to whiteness in a charcoal crucible, it is reduced, and gives a porous, gray, unfused mass, which may be compressed, and has then the colour and lustre of plumbago.

Alloys of Vanadium.—This branch of the history of vanadium remains to be investigated. M. Sefström, who is principally occupied with metallurgic researches, intends to make it the subject of extensive investigation. The experiments which I have had an opportunity of making on the subject prove that vanadium combines readily with other metals; it is sufficient to fuse by the blowpipe on charcoal several metallic vanadates, to reduce them to the state of alloys of vanadium; but in this case they are deprived of ductility. In experiments upon vanadium, the surface of platina crucibles is often alloyed with vanadium, which does not alter either the colour or the metallic lustre of the platina; but when it is afterwards heated to redness, the alloyed parts are covered with a layer of fused vanadic acid which preserves them from further oxidation. When they are heated to redness, and afterwards washed with potash (and this is repeated five or six times), the vanadium is separated. Neither bisulphate of potash nor borax mixed with nitre when melted in the crucible succeed so well. I have not found that the crucibles were in any way injured.

*Salts of Vanadium**.—The salts which contain oxide of vanadium as a base are, with few exceptions, of a superb azure blue colour when in solution. In the solid state and combined with water, they are either of a deep or light blue colour, and sometimes greenish. Without water they are generally brown, and sometimes also green. Both the brown and green salts give blue solutions. Their taste is astringent and rather

* In this translation we have entirely neglected Berzelius's newly invented nomenclature. It has increased the difficulties of the science, without adding one advantage, that we can perceive.—Edit.

sweetish,

sweetish, like those of iron. The greater number of them are soluble in water; the caustic alkalies occasion a precipitate, which is at first of a grayish white colour, and which afterwards becomes of a liver brown; an excess of alkali dissolves the precipitate, producing a solution of a brown colour. Ammonia added in excess gives a brown precipitate, and the liquid becomes colourless. The carbonates occasion grayish-white precipitates: sulphuretted hydrogen does not render them turbid, but the hydrosulphurets occasion a black precipitate, and when added in excess they redissolve it, occasioning a fine purple colour; ferrocyanate of potash occasions a lemon-yellow precipitate, which becomes green in the air. Infusion of galls gives a precipitate of so deep a blue colour that it appears black.

Chloride of Vanadium.—This salt has not been obtained in an anhydrous state: it can only be formed by passing the vapour of chloride of vanadium over a mixture of suboxide of vanadium and charcoal heated to redness: when sulphate of vanadium, dried as much as possible, is mixed with chloride of potassium and fused, the mass contains nearly the whole of the vanadium. It appears that the vanadium is converted into acid at the expense of the sulphuric acid.

Chloride of vanadium (muriate?) is very easily obtained in combination with water. If vanadic acid is dissolved in concentrated muriatic acid and heated, chlorine is evolved; but when it is digested with suboxide of vanadium or with vanadium, chloride is obtained free from chlorine. The same effect is produced by adding to the solution a little sugar, sulphuretted hydrogen, or alcohol. The solution has a fine blue colour. If, on the contrary, concentrated muriatic acid be poured upon oxide of vanadium prepared by calcining vanadate of ammonia in close vessels, a brownish black solution is obtained, and a little chlorine is disengaged, owing to the decomposition of a little vanadic acid. The brown liquid, saturated as perfectly as possible with oxide, suffered to evaporate spontaneously, becomes concentrated to a certain point, but is not rendered dry. Diluted with water, it retains its brown colour; but when evaporated with heat, it becomes gradually completely blue. This change takes place immediately when sulphuric acid is added, even to the concentrated liquid; and in this case neither precipitation nor the evolution of any gas occurs. It appears that the brown chloride differs only from the blue, in being in a different isomeric state, which the sulphuric acid instantly changes. The blue chloride gradually concentrates, and in thin layers it dries, leaving a brown varnish, which is no longer perfectly soluble in water. Evaporated at a mode-

rate heat, it is entirely converted into this brown mass, which is a subchloride. It gives no appearance of crystallization. Concentrated chloride of vanadium may be mixed with absolute alcohol without precipitation. Ammonia occasions a greenish gray precipitate, which may be washed without dissolving, and which appears to be a subchloride containing ammonia.

Bromide of Vanadium.—This salt resembles the blue chloride in all respects. Hydrobromic acid dissolves anhydrous oxide of vanadium, the solution is blue. The concentrated bromide, mixed with absolute alcohol, becomes in a few seconds gelatinous, because the alcohol seizes the water; but it becomes liquid in proportion as the alcohol evaporates. When dried it becomes brown, but it redissolves almost entirely in water. Ammonia precipitates a greenish gray double chloride.

Iodide of Vanadium.—Its solution is blue like that of the preceding salts, but it becomes quickly green in the air. By spontaneous evaporation it becomes a semi-fluid brown mass, which when diluted is of a blackish brown colour. Sulphuric acid then disengages iodine. It appears to contain a mixture of vanadate of vanadium and of ioduretted iodide of vanadium. I have not examined it very minutely.

Fluoride of Vanadium.— In solution it is blue, and when dried brown, and redissolves in water. By spontaneous evaporation it becomes greenish and of a syrupy consistence, and greenish crystals are formed. In this state it is soluble in absolute alcohol, which does not restore its original blue colour; sulphuretted hydrogen easily produces this effect. This fluoride combines with the alkaline fluorides, with which it forms salts of a light blue colour, very soluble in water, but not in alcohol.

Fluo-silicate of Vanadium.—The solution is blue; when evaporated at 140° Fahrenheit it gives a spongy light blue coloured mass. By evaporation it becomes green, and resembles the fluoride.

Cyanuret of Vanadium.— When hydrate of vanadium is treated with hydrocyanic acid, it becomes brown, and it may be washed without becoming green or dissolving. I afterwards treated it with cyanuret of potassium; it dissolved, but the solution left to spontaneous evaporation only gave vanadate of potash, exhaling at the same time the odour of hydrocyanic acid. The ferrocyanate of vanadium precipitates in a bulky mass, of a fine lemon colour with a greenish tint. It is not soluble in diluted acids; by exposure to the air it becomes of a fine green; the perferrocyanate of vanadium precipitates in a gelatinous mass of a yellowish green.

Sulphate

Sulphate of Vanadium.—This salt is obtained by dissolving vanadic acid or oxide (as obtained by the calcination of vanadate of ammonia) in sulphuric acid, mixed with an equal quantity of water, and by passing sulphuretted hydrogen into the solution diluted with water, in order to reduce the last traces of the vanadic acid dissolved. Oxalic acid may be used for the same purpose; the liquid is to be evaporated until the excess of sulphuric acid begins to concentrate; the salt is then deposited in the form of a transparent crystalline crust of a dirty blue colour. The excess of acid is to be drained from the salt, and then it is to be washed with absolute alcohol. The salt gradually swells, and is reduced to a light crystalline powder of an ultramarine blue colour; it is then washed with alcohol, which it always renders of a blue colour, although but a very small quantity is dissolved. It is afterwards dried by being placed under a receiver, with a vessel containing snlphuric acid or chloride of calcium. An essential difference appears to exist between the salt which crystallizes in the concentrated acid and the blue pulverulent salt; but I do not know in what it consists. It is probable that the former is a supersalt; for the blue powder which I analysed is neutral sulphate. In this state sulphate of vanadium appears but slightly soluble in cold water; it first diffuses itself through it, and dissolves with extreme slowness, but in hot water it dissolves readily: the supersulphate is deliquescent in warm moist air, and becomes of a syrupy consistence; while the same quantity of sulphate, kept under water, remains almost entirely insoluble. It is rather difficult to obtain this salt in regular crystals.

The best method of crystallizing it is to let the dry sulphate deliquesce, and to suffer it to remain for some weeks. A very slight excess of acid often favours the experiment, which never succeeds in moist weather. The crystals consist principally of an aggregation of prisms; but I obtained some which were very short simple right prisms with rhombic bases, having small triangular oblique facets at the summit of each acute angle. Their colour resembles the fine blue of sulphate of copper, but is perhaps rather deeper. This salt contains 17·9 per cent. of water, the oxygen of which is to that of the base as 2 to 1. This also is the composition of the powder precipitated by alcohol. Sulphate of vanadium is decomposed by heat; the oxide is converted into vanadic acid at the expense of the sulphuric acid; sulphurous and anhydrous sulphuric acid are disengaged, and fused vanadic acid remains. If hydrate of vanadium be digested in a slightly concentrated solution of the sulphate, the hydrate is dissolved, and

and a soluble subsalt is obtained, which dries by spontaneous evaporation into a blue transparent varnish, which becomes brown and loses water when heated to 212° Fahrenheit. The water redissolves it; but the solution exposed for a long time to the influence of the air becomes green, and the salt is eventually converted into vanadate of vanadium, which is deposited, and leaves the neutral blue salt in the state of a concentrated solution.

Sulphate of Vanadium and Potash.—This is obtained by mixing, in proper proportions, the solutions of the two neutral salts. The double salt does not crystallize, but dries into a gummy mass, of a light blue colour, which contains no trace of crystallization.

Nitrate of Vanadium.—Nitric acid dissolves vanadium, its oxide and suboxide; the solution has a blue colour, which is not altered by boiling: but when hydrate of vanadium is dissolved to saturation in nitric acid, and the solution is suffered to crystallize spontaneously, it becomes green when it has acquired a certain degree of concentration, and at the instant of complete desiccation the acid decomposes and leaves vanadic acid, which retains a small quantity of combined nitric acid.

Phosphate of Vanadium.—The neutral salt gives a blue syrup, which does not crystallize, and which, when dried with heat, becomes white and swells like alum dried in the fire. At a white heat it becomes round and agglomerates, but it does not fuse. It is then of a deep colour, and completely insoluble in water. The phosphate may be obtained in very small blue crystals by mixing it with a certain excess of phosphoric acid, and evaporating the solution at the temperature of 123° Fahrenheit. After some time the neutral salt is found crystallized in the midst of a colourless mother-water, which is merely concentrated phosphoric acid; it may be afterwards removed by alcohol. In the air these crystals rapidly deliquesce; when a concentrated solution of phosphate of vanadium is mixed with absolute alcohol, a grayish blue gelatinous precipitate is formed, which when washed with alcohol and dried is almost white, and does not alter in the air. It does not completely dissolve in water, and appears to be a subsalt.

Arseniate of Vanadium.—A solution of this salt containing an excess of arsenic acid gives by evaporation a crust composed of small crystalline grains of a light blue colour, which may be deprived readily of the excess of acid by washing with water. It dissolves so very slowly even in hot water acidulated with arsenic acid, that it might be thought insoluble; water, nevertheless, is capable of holding much of it in solution. Muriatic acid dissolves the crystals readily. If arsenic acid

acid be saturated with hydrate of vanadium, a very concentrated solution is obtained, which furnishes by evaporation a crystallized neutral salt, and also a gummy mass which appears to be a subsalt. Alcohol acts upon the arseniate in the same manner as upon the phosphate.

Borate of Vanadium.—A solution of borax precipitates the salts of vanadium of a brownish gray colour. This precipitate is dissolved by an excess of boracic acid. The solution is blue, but it soon becomes green in the air. If an attempt is made to restore the blue colour by passing a current of sulphuretted hydrogen into it, oxide of vanadium and subsulphuret are formed, which remain dissolved in the boracic acid, and colour the solution of an intense brown. Sulphuric acid quickly precipitates this solution, and the precipitate consists of subsulphuret of vanadium. Subjected to spontaneous evaporation in the air, the solution soon becomes green, and leaves at last a solid mixture, of a greenish brown colour, of sulphate and vanadate of vanadium, sulphur, and boracic acid in crystalline scales.

Carbonate of Vanadium.—It appears that this salt cannot be obtained in the moist way. I have stated above that the precipitate formed by the alkaline carbonates in the salts of vanadium consists of hydrate free from carbonic acid, or contains only traces of it; the presence of carbonic acid depends upon accidental circumstances, as happens with the oxides of cobalt and nickel.

Silicated Vanadium.—Prepared by double decomposition; this salt forms a greyish precipitate, which becomes green by drying. Water does not separate anything from the green powder.

Molybdate of Vanadium.—When sulphate of vanadium is mixed with molybdate of ammonia, both in solution, a liquid of a fine purple colour is obtained similar to that of tungstate of molybdenum. On this account I imagined that there was an exchange of oxygen, and that vanadate of molybdenum was formed. But when a molybdate is mixed with vanadate of ammonia, it becomes yellow and not purple. The purple colour disappears gradually in the air; it is at first replaced by blue, then by green, and lastly by yellow, without the solution becoming turbid.

Tungstate of Vanadium.—This salt is precipitated in the form of a brownish yellow powder. It dissolves in a sufficient quantity of water, and when it is left in the liquid it dissolves without the addition of water, in proportion as the oxide of vanadium acidifies. The solution eventually contains a yellow combination of the two acids.

Chromate

Chromate of Vanadium.—Chromic acid dissolves hydrate of vanadium. The solution is of a brownish yellow colour, and by spontaneous evaporation leaves a brown polished varnish, without any trace of crystallization. This varnish does not completely redissolve in water, and the fresh solution is yellow. Sulphuretted hydrogen precipitates a greenish mass, and the liquid becomes colourless.

Oxalate of Vanadium.—Oxalic acid saturated with hydrate of vanadium, and evaporated, gives a blue varnish, which is transparent and dissolves slowly in cold water, but more readily in warm water. This salt mixed with oxalic acid gives a blue crystalline salt, which is very soluble in water. It remains to be determined whether the first is a neutral or subsalt, and whether the second is, as it appears to be, a supersalt.

Tartrate of Vanadium.—This salt is of a light blue colour, the remarkable beauty of which is derived from its not becoming green by contact with the air. In the dry state it forms a transparent splintery mass, which requires many hours for solution in cold water, but hot water dissolves it more readily. Ammonia dissolves it; the solution has a fine purple colour, which it loses by the acidification of the oxide of vanadium when it is exposed to the air.

Oxalate of Vanadium and Potash.—This salt gives a blue mass, which does not crystallize. Oxalic acid and the binoxalates decompose vanadic acid, and form blue salts of vanadium.

Tartrate of Vanadium and Potash. — This salt has the appearance of a splintery extractive mass; its colour is blue with a strong shade of violet. It may be obtained also by dissolving vanadic acid by means of bitartrate of potash: in this case part of the tartaric acid is decomposed. Ammonia does not precipitate the double tartrate, but it imparts to it a magnificent purple colour, which is destroyed by the action of the air.

Citrate of Vanadium.— This salt is uncrystallizable, and gives a splintery mass of a very deep blue colour. It redissolves slowly in cold water; the solution is of a pure blue colour. Ammonia dissolves it, and assumes a yellowish brown colour, which gradually disappears by oxidation when exposed to the air.

Acetate of Vanadium. — Dilute acetic acid dissolves very little hydrate of vanadium. The liquid is of a pale blue colour, and yields a deposit when suffered to evaporate spontaneously; this is a white powder, whilst the excess of acid evaporates. When evaporated in a stove it becomes green, and the residuum

duum is no longer soluble even in concentrated acetic acid. Acetate of potash does not precipitate the salts of vanadium. Concentrated acetic acid dissolves hydrate of vanadium, but the solution becomes green by spontaneous evaporation, and leaves a granular powder, composed of deep green opake microscopic crystals, the form of which is either a cube or a very short rectangular prism; they dissolve very slowly in water; the solution is of a deep green colour.

Formiate of Vanadium.—Artificial formic acid dissolves hydrate of vanadium, and gives by spontaneous evaporation a blue, opake, saline mass, which is easily soluble in cold water; but this solution, which contains no excess of water, becomes gradually green by contact with the air. The salt when perfectly dried is of a violet colour, with a tint of brown, and is not completely soluble in water.

Succinate of Vanadium.—Succinic acid dissolves very little hydrate of vanadium; the solution is but slightly coloured. By evaporation the succinate is obtained in the state of a white powder, mixed with crystals of succinic acid. Nevertheless, the salts of vanadium are not precipitated by neutral succinates, but the mixture soon becomes greenish.

Benzoate of Vanadium.—A boiling solution of benzoic acid dissolves a little hydrate of vanadium; submitted to a slow evaporation the solution deposits a yellow powder, and the excess of acid crystallizes around it.

[To be continued.]

XLII. *Particulars of the Measurement, by various Methods, of the Instrumental Error of the Horizon-Sector described in* Phil. Mag. vol. lix. *By* John Nixon, *Esq.*

[Continued from page 96.]

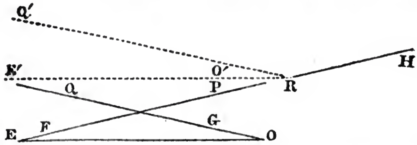
By the Eleventh Method.

Theory.—LET E (next page) represent the eye end, and O the object-glass end of the (horizontal line) of collimation of a perfect telescopic-level (or of the horizon-sector) correctly adjusted for taking a level. If we increase the diameter of the cylindrical ring nearest the object-glass, by for instance coiling a quantity of thread about it, the line of collimation will be elevated in the direction FP; and on reversing the telescope within its Ys, it will be equally elevated in the opposite direction, or GQ*; the angle PEO = QOE being

* Admitting the Ys to have the same angular opening; but it is evident from the demonstration given in page 429 of last volume, that were the Ys unequal the theory would be equally applicable.

the

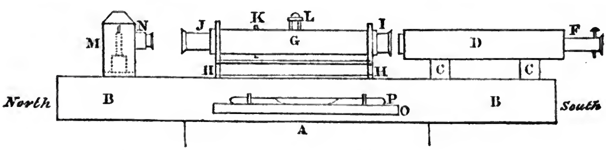
the instrumental error. Then if we place a second or proof telescope with its object-glass nearly in contact with that of



the imperfect telescopic-level P, and make their lines of collimation parallel to each other, we shall find, on reversing the latter, that the line of collimation of an object-glass substituted for the eye-tube E will be inclined to that of the proof telescope HR by *twice* the instrumental error. As the rays of light from the intersecting point of the cross wires of the telescopic-level pass out of the additional object-glass into that of the proof telescope parallel to each other, make Q′R parallel to QO, and Q′RE will be double the angle PEO = QOE. Or, more simply, if we make E′O′ parallel to the horizontal line EO, the line QR is elevated, and RE depressed by an angle equal to the instrumental error.

Description of the Apparatus.

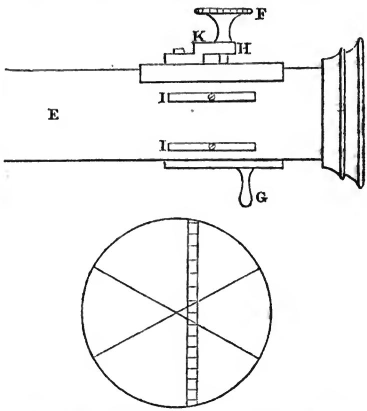
On an oak table (A) 3·5 feet long and 2 feet broad, which stood against the eastern wall of a long room having but one



window (to the south), was placed a deal plank (B) 6 feet long, 3·5 inches broad, and very nearly 6 inches thick, or high; equal lengths of the plank overhanging each end of the table. To prevent the plank upsetting, a bar of wood about a foot long and 3 inches square, was fixed to each side of it, their bases being slightly elevated above that of the plank, that the latter might not bend by the bars coming in contact with the table.

Two blocks of wood (CC) 3·5 inches high, 2·5 inches broad, and of the length of the breadth of the plank, were glued to its upper surface, one at a distance of 6 inches, and the other at that of 17 inches from its south end. Firmly glued to these blocks, stood an achromatic telescope set in a stout mahogany

mahogany frame (D) about two feet long and 2·5 inches *square*, each end of the frame projecting equally beyond its subjacent block. The sidereal focus of the object-glass was full 20 inches, forming with the eye-tube a total focal length of nearly 30 inches. Within the eye-tube, between the first and second glasses, was placed a stop containing a vertical slip of very thin mother-of-pearl divided on one edge by fine horizontal lines into equal parts of about 1′ 20″ each, together with two fine spider threads crossing each other, a few seconds to the east, and in the plane of the slip of pearl, at an inclination to the horizon of about 30°. By means of the micrometer screw (H), worked by the milled head-nut (F) placed outside the tube, the intersecting point of the threads could be gradually moved upwards or downwards, parallel to the divided edge of the *fixed* slip of pearl. The handle (G) served to slide the whole apparatus, parallel to the sides of the eye-tube, to that distance from the eye-glass at which the threads appeared most distinct. Four screws, with sliding slips of brass beneath their heads (of which two (I I) are represented in the diagram), were used in the usual manner to alter the situation of the point of intersection of the threads.



The proper object-glass (I) of the sector, fitting tightly within the thicker end of the cylinder, is 13·1 inches in focal length, and protrudes about 2 inches beyond the cylinder. Within the opposite end of the cylinder, the eye-piece being withdrawn, was fixed an additional object-glass (J) of 10·3 inches focal length, protruding about 4 inches. The stop containing the (adjustable) horizontal and vertical wires is fixed within the cylinder, and was made the joint or common focus of the two object-glasses. The sector being placed about the middle of the length of the plank, with its cylinder nearly on a level, and in a line with the telescope (D), the stand of the sector was moved laterally until the vertical wire of the

 cylinder,

cylinder, as seen through the telescope, appeared to be bisected by the intersecting point of the threads of the latter. This being effected, the stand was glued at the sides (only) to the surface of the plank, and the slight deviation in the bisection of the wire, occasioned by the setting of the glue, rectified by the lateral adjusting screws (I I) of the telescope.

Illumination of the Wires.—In the daytime a square foot of white pasteboard set up on the plank at a distance of about six inches from the northern object-glass of the cylinder, was so inclined as to reflect the light from the window up the cylinder and telescope. At night, or on a gloomy day, the lantern (M) of a transit instrument was substituted, and so situated that the tube (N) containing the lens was of the height and in a line with the cylinder and telescope. In lieu of a lamp, a white wax taper, about one-third of an inch thick and five inches long, was fixed within the socket of the lantern, the summit of the taper standing an inch or two above the level of the lens-tube (N). The wires of the cylinder and threads of the telescope could now be seen, on looking through the eye-glass of the latter, with the utmost distinctness in front of the pale taper mildly illuminated by the light reflected from the tinned sides of the lantern. When the taper had burnt down nearly to the level of the lens-tube, the light became so intense that the wires appeared to split; and, on the other hand, when the taper stood too high, they looked dim and ill defined. In general, whether the lantern or pasteboard was used, a piece of black cloth was thrown loosely over the object-glass of the telescope and the adjacent one of the cylinder, its loose folds being so arranged as to exclude all light, except that which passed through the further object-glass of the cylinder.

Adjustment of the Wires to the Sidereal Focus.—The telescope, previously to its being glued to the plank, was pointed, when the ground was covered with snow, to some park railings at a distance of several miles, and the eye-tube pushed in or drawn out until the railings could be seen with the greatest distinctness*. The telescope being then considered as properly adjusted, the spider threads were moved by the handle (G) to such a distance from the eye-glass as produced the clearest vision of the minute particles of dust adhering to their sides. To confirm the accuracy of the latter adjustment, the lantern was placed a little beyond the eye-glass, and the threads viewed through the object-glass by an excellent 20-inch (*military*) achromatic by Dollond, of which the sidereal

* The telescope could not conveniently be adjusted upon a star.

focus

focus had been ascertained in the way described. On commencing the verification, the latter was placed, purposely, out of focus, but when altered so as to produce distinct vision of the threads, its eye-tube was found, on every trial, to be at the circle marking the sidereal focus.

The object-glasses within the cylinder were placed at a distance from the cross wires fixed between them equal to their respective sidereal foci (the cylinder and telescope being situated as represented in the figure, p. 338) by drawing out or pushing in the tube containing the proper object-glass of the cylinder, until the wires, as viewed through the telescope, appeared as distinct as possible. The cylinder being reversed, the additional object-glass, now brought close to that of the telescope, was similarly adjusted.

(*New*) *Method of setting a Wire vertical.*—The uppermost (L) of the transverse levels of the cylinder having its bubble at the mark, the slip of pearl had its divided edge rendered parallel to the upright wire of the cylinder by unscrewing in a very slight degree the eye-tube containing it*. The cylinder was then reversed within its Ys, and the bubble of the transverse level brought to its mark by turning the cylinder within its Ys. On looking through the telescope, if the slip and wire were found parallel, both were considered as perpendicular; otherwise one half of the deviation was corrected in the slip and the other in the wire, the bubble of the deranged level being in the last place restored to its mark by its adjusting nuts. After the cylinder had been *inverted* and the wire placed parallel to the (undisturbed) slip, the other transverse level was similarly adjusted. In setting the lines of collimation parallel, or in reading off the great levels during the subsequent observations, the bubble of the transverse level uppermost was carefully kept to its marks.

Reference-Level.—The horizontal piece of brass (K) in which works that part of the screw situated between its head and the threads was so thin as to be elastic, thus rendering the measurements made with the micrometer discordant and uncertain. Recourse was therefore had to the great levels of the sector (omitted in the figure), not however without reluctance, the unsteadiness of the floor now requiring the addition to the apparatus of a reference-level. To the upper surface of the westernmost (O) of the pieces of wood projecting from the sides of the plank was fixed a bar of oak (P) with its eastern side glued to the plank. The bar was mounted with a spirit-

* The slip being only a few seconds distant from the intersection of the threads, the eye could estimate the parallelism with sufficient accuracy.

level

level tube (having a scale of $\frac{1}{80}$th of an inch for 1″) placed parallel to the sides of the plank; and no measurements were made by the great levels until the ends of the bubble of the reference-level were brought, by an alteration of the situation of the observer, to be stationary between two certain divisions of its scale.

(*New*) *Verification of the Adjustments of the Foci.* — It has already been stated that the cylinder and telescope were placed on the plank in a line, and *level* with each other. In this position, before the stand of the sector was glued to the plank, the line of collimation of the telescope was made parallel to that of the *proper* object-glass of the cylinder (that is, the intersection of the wires of the cylinder appeared to be in a line with, or intercepted by the intersection of the threads of the telescope); when first one and then, on inverting the cylinder, the other great level were carefully read off. In the next place, the stand of the sector was taken off the plank, and replaced with a board 0·85 inch thick, introduced between it and the plank. The cylinder being now raised nearly an inch above the level of the telescope, their lines of collimation were once more made parallel, and the great levels again read off*. The results were:

	Bubble.		Bubble.
Sector-stand placed on plank; right hand level	42°; 113°	Left do.	33°; 123°
Ditto on removal of *board*...	$40\frac{1}{2}$; $130\frac{1}{2}$	——	$31\frac{3}{4}$; $122\frac{3}{4}$
Mean	$41\frac{1}{4}$; $131\frac{3}{4}$	——	$32\frac{1}{4}$; 123
Sector-stand placed on *board*	41; 131	——	33; 123

(The divisions of the scales are about 2″ each.)

Flexibility of the Plank.—As one end of the cylinder was very probably a few ounces heavier than the other, the distribution of gravity might be *sensibly* varied, in case the flexure of the plank was considerable, on reversing the cylinder within its Ys. As a two-pound weight could however be placed on any part of the plank without affecting the parallelism of the lines of collimation, its flexibility was undoubtedly too slight to vitiate the measurements. But on fixing a se-

* With Kater's horizontal floating collimator the parallelism of the rays might be elegantly demonstrated. Point the wires of a telescope having a very large object-glass exactly at those of the collimator, the telescope being placed as much as possible above the level of the collimator without losing sight of its wires. The tube of the collimator may now be raised several inches in level by gradually augmenting the quantity of quicksilver within the trough, yet without disturbing the apparent interception of the wires of the collimator by those of the telescope.

cond

cond telescope at the northern extremity of the plank as a counterpoise to the one at the opposite end, and introducing a narrow bar of wood between the table and the plank, on which the latter would altogether rest without touching the table, the cross of the threads of the telescope, which had been previously set to bisect the horizontal wire of the cylinder, was found to have started from it to the amount of several seconds.

Process of Measurement. — The line of collimation of the proper object-glass of the cylinder having been made parallel to the axis of the latter (which was easily and most accurately effected by the cross threads of the telescope), it was then made parallel to that of the telescope by elevating or depressing the cylinder by means of the rack-work of the stand, rather than by the more expeditious, yet uncertain application of the micrometer, which, with a view to insure its steadiness, had been long undisturbed. In the next place, by reversing the cylinder within its Ys, the additional object-glass was brought close to that of the telescope, when its line of collimation was found to be so nearly parallel to that of the telescope, that it could be rendered *level* with it by unscrewing a little the additional object-glass, which was not well centred, within its cell. This effected, the cylinder was inverted within its Ys, and on looking through the telescope, the intersection of the wires of the cylinder appeared above the level, and considerably to the right of the point of intersection of the threads of the telescope. To measure the difference of *level* of these two points of instersection, evidently equal to the *quadruple* of the instrumental error, both the great levels were read off; then the horizontal wire of the cylinder being brought by the rack-work of the stand to the level of the intersecting point of the threads of the telescope (by which it appeared to be bisected), the reading off of the levels was repeated. Example, February 9th, 1830.

Cylinder reversed; additional object-glass and that of telescope together.

	Bubble.
Right-hand level (scale, $1° = 1''\cdot75$)	$46\frac{1}{4}°$; $136°\frac{1}{4}$
Left-hand level (scale, $1° = 1\cdot92$)	50 ; 141

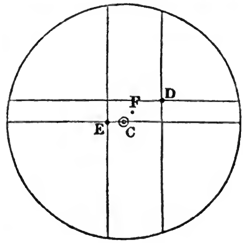
The line of collimation of the telescope level with the horizontal wire of cylinder.

	Bubble.
Right-hand level	$19°\frac{1}{2}$; $109°\frac{3}{4}$
Left-hand level	$25\frac{1}{2}$; $116\frac{1}{2}$

To confirm the measurements the cylinder was again reversed, which brought the proper object-glass of the cylinder close

close to that of the telescope. The levels were then noted before and after the parallelism of the lines of collimation had been effected.

When the line of collimation of the proof object-glass had been made parallel to, or more correctly, *level* with that of the telescope by unscrewing it within its cell, the cross wires of the cylinder intersected each other at E, about 1′ to the *west* of C, or point in which they were situated previously to reversing the cylinder; but on turning the cylinder half round within its Ys, the intersection was removed to D, about 11′ *east* of C. The axis of the (reversed) cylinder, situated at F, must therefore have been about 6′ out of its previous direction; a consequence either of the Ys being of an irregular figure, or of the cylinder not being straight. Two systems of cross wires, one for each object-glass, would certainly have been preferable.



Having discovered that the tube containing the additional object-glass did not fit sufficiently tight within the cylinder, beyond which it projected considerably, the evil was remedied by applying glue to fix it. Still there existed some degree of doubt whether the tube was not sensibly deflected by its own weight.

The average of twenty-five measurements in April 1829, and January and February 1830, with the room constantly at 45° Fahr., made the instrumental error between 12″·0 and 16″·6, mean 13″·8. Those of 1829 were made before the present object-glass of the cylinder was substituted for the original one of greater focal length, on a plank 5·5 feet long, 5 inches broad, but only 3 inches thick. The average of these (four) measurements was 14″·1.

By the Eleventh Method varied.—With an efficient micrometer, a more simple or accurate method than the one just described could scarcely be devised. However, as the error was of necessity to be determined by the spirit-level, the measurements were repeated with such improvements in the apparatus and variations of the method as promised the most successful results.

Two iron brackets, driven, at a distance of 30 inches from each other, into a seam of the stone wall forming the east side of the room, and from which they projected nearly a foot, supported

supported a deal plank $4\frac{1}{2}$ feet long, 5 inches broad, and 3 inches thick, secured to them at a distance of 4 inches from the wall *.

To the upper surface of the plank, planed as horizontal as possible, was glued towards its northern end the stand of the sector. The cylinder contained, as before, its own and an additional object-glass, but the latter was more effectually fastened within it at the slit used for the eye-tube by two broad-headed screws driven in at some distance from each other. South of the sector stood the telescope, *moveable*, with the two blocks of wood to which it was glued, along the surface of the plank. Into its upper surface were screwed parallel to its sides the brass Ys of the proof-level, carrying Fortin's level-tube.

As the plank did not prove *perfectly* steady, a mahogany bar, mounted with two brass Ys supporting a level-tube, was glued to its surface parallel and nearly close to the western side of the sector-stand. The tube, 14 inches in length and one in diameter, had a scale of $\frac{1}{16}$th inch to $2''$. With the aid of this reference-level the minute inclination given to the plank by moving the telescope along it could be detected, and readily corrected by placing one or more light weights upon a proper part of the plank.

As the axis of the reversed cylinder could not be made parallel to the line of collimation of the additional object-glass without disturbing the cross wires of the cylinder, it was of the utmost importance that one of the latter, when the bubble of either transverse level stood at its marks, should be truly horizontal. After this had been effected as correctly as was practicable by the former plan, the telescope was pointed first at one and afterwards at the other extremity of the wire; Fortin's level being noted in both instances. The inclination of the line being proved by the slight difference observed in the two readings, it was gradually altered until it would endure this severe test. The cylinder being inverted, the other transverse level underwent a similar scrutiny.

As the pearl slip was of unquestionable steadiness, one of its divisions was substituted for the cross threads of the telescope, and could be brought with rigid accuracy to be in a line or level with the horizontal wire of the cylinder, by gradually withdrawing from beneath one of the blocks fixed to the telescope a sheet of thin paper previously introduced between the plank and block. At the same time the vertical wire of the cylinder, as dimly seen through the diaphanous pearl, divided the slip into two equal vertical sections.

* Suggested by Captain Kater in his description of the floating collimator.

Method of Observation.—The telescope being in a line with and of the height of the cylinder, with the proper object-glass of the latter at a short distance from that of the telescope, the division of the pearl slip was got in a line with the horizontal wire of the cylinder, before, and (as an additional precaution) after the cylinder had been inverted; the reading of Fortin's level following each observation. The cylinder being reversed, the telescope was moved southwards until its object-glass stood at the proper distance from the additional object-glass; when the process of making the division of the slip level with the wire previous and subsequent to inverting the cylinder, together with the simultaneous noting of Fortin's level, was repeated. (It is almost superfluous to state that the latter was never read off until the bubble of the reference-level stood between its marks.)

There were now sufficient *data* to determine the difference of inclination of the axis of the cylinder resulting from the latter being reversed within its Ys; which difference it has been demonstrated is equal to double the instrumental error.

To prove that the great excentricity of the additional object-glass did not vitiate the results, the two last measurements (marked in the subjoined list with an asterisk) were made with the line of collimation of the additional object-glass rendered parallel to the axis of the cylinder, on reversing it within its Ys, by altering the cross wires with the utmost care.

List of the Results of the Measurements.

April 8th, 1831.	Temp. 57°.	Error =	13″·2
—— ——	—— 57	——	13 ·0
—— ——	—— 58	——	13 ·7
—— ——	—— 59	——	11 ·6
April 9th, 1831.	—— 56	——	13 ·2
—— 11th.	—— 62	——	12 ·4*
—— ——	—— 63	——	13 ·3*

The mean of the whole is 12″·8, but if we reject the fourth measurement it will become 13″·1.

By the Tenth Method.

Theory.—Having demonstrated that the axis of the faulty cylinder deviates, when reversed, from the opposite of its previous direction by double the instrumental error, it is evident that the line of collimation of a telescope made parallel to the axis of the cylinder *before*, and that of another telescope made parallel to it *after* the reversing took place, would be inclined to each other by twice the error.

Apparatus.—In addition to the apparatus first employed, two

two substantial blocks of wood were glued, nine inches asunder, to the upper surface of the plank towards its northern end; (occupying the place of the lantern as represented in the figure, p. 338.) These blocks, notched in the middle in a line with the cylinder and telescope, supported, on a level with the two latter, a tube of wood 14 inches long and 1·5 inch in diameter, containing within its southern end the additional object-glass and tube, and a fine eye-piece (by Dollond) within the northern end. Distinct vision of the cross lines, *fixed* at right angles to each other, between the first and second glasses of the eye-tube, being obtained by regulating their distance from the moveable eye-glass, the object-glass was adjusted to the proper focus by pointing the tube (telescope) at the sails of a distant windmill.

Method of Observation.—The cylinder being removed, its place within the Ys was supplied by a tin tube (of the same diameter) blackened within, which reached so nearly from the object-glass of one telescope to that of the other that all false light could be excluded by a large piece of black cloth covering the tube and object-glasses. Having placed the lantern a little beyond the eye-glass of the *round* telescope, the intersecting point of the cross threads of the *square* telescope were moved by their micrometer screw until they bisected the horizontal wire of the round telescope where crossed by the vertical one. Removing the lantern to beyond the eye-glass of the square telescope, the bisection, on looking through the round telescope, did not appear quite perfect or certain, being *slightly* affected by evident parallax.

Lastly; the cylinder containing its proper object-glass only, being substituted for the removed tin tube, the line of collimation of its object-glass, which was situated close to that of the round telescope, was made parallel by the rack-work of the stand to that of the latter; or rather, as nearly so as the almost impossibility of placing one wire exactly before another would admit of. The cylinder being reversed, the great levels were read off, before, and then again after the line of collimation of its object-glass had been made parallel, by the rack-work of the stand, to that of the square telescope. In making the bisections, the lamp stood on separate apparatus near the eye-glass of the opposite telescope.

The results (scarcely worth transcribing) varied in eighteen measurements from $15''\cdot5$ to $27''\cdot5$; mean $21''\cdot3$, or $8''$ more than by the *Eleventh* method.

JOHN NIXON.

[To be continued.]

 XLIII. *Notice*

XLIII. *Notice on Oxalic Acid.* *By* Edward Turner, *M.D. F.R.S. L. & E. &c., Professor of Chemistry in the University of London**.

IN a former Number of this Journal† I had occasion to publish some remarks on the volatility of oxalic acid; and about the same time M. Gay-Lussac published in the *Annales de Chimie et de Physique*, vol. xlvi. p. 218, a short memoir on the easy decomposition of oxalic acid by the agency of heat‡. In my notice the sublimed acid is described as supporting a temperature of 330° Fahrenheit, without any decomposition; while M. Gay-Lussac, speaking of the crystals in their ordinary state, describes them as being decomposed at so low a temperature as 230° Fahrenheit. As these statements may be thought contradictory, while at the same time a correct knowledge of these facts is required for understanding an interesting point of theory,—the action of the sulphuric on oxalic acid,—I have re-examined the subject with the hope of correcting any inadvertence which may have been committed.

In the experiments which I have made with this view, the heat was applied through the medium of a small mercurial bath, heated by a lamp: the acid to be decomposed was contained in a small glass tube, the sealed end of which was plunged into the bath, and its other extremity connected in the usual manner with a mercurial trough. The temperature was ascertained by fixing the bulb of a thermometer in the bath during the whole continuance of the experiments. By operating in this way I found that my former statement was strictly correct. Oxalic acid containing only one equivalent of water, whether prepared by merely heating the ordinary crystals or by sublimation, sustained a temperature of 330° without yielding either water or gas. As the thermometer rose from 330° to 340°, gas began slowly to appear, and at 370° it was freely disengaged. It hence follows that the best temperature for subliming oxalic acid is 330°: it then sublimes with rapidity, and yet there is no loss by decomposition; but before exposure to this degree of heat, the acid should previously be dried as much as possible at a lower temperature.

But it does not necessarily follow, because my statement is correct, that that of M. Gay-Lussac should be erroneous. On the contrary, though we differ as to the precise degree of heat at which decomposition takes place, I find that crystallized oxalic acid, containing one equivalent of real acid and three of

* Communicated by the Author.

† Phil. Mag. and Annals, N.S. vol. ix. p. 161.

[‡ See also Phil. Mag. and Annals, vol. x. p. 153.—Edit.]

of water, is decomposed by heat at a lower temperature than the same acid after two equivalents of water have been removed. Thus, on operating with the fully hydrated acid in the same manner as with that which had fully effloresced on the sand-bath, fusion took place at 209°, as stated by M. Gay-Lussac, and not at 220° as I had found in heating some crystals, which must previously have lost a little of their water of crystallization. As the temperature of the bath rose to 230°, the fused mass remained quite tranquil, yielding a little water, but not a particle of gas. At 240° there was still scarcely any gas, and very little at 250° or 270°. Even at 290°, though there was violent ebullition from the rapid escape of aqueous vapour, yet the evolution of gas was by no means abundant: it became free at 310°, and was rapid at 320°. In a second careful experiment the results were precisely similar. It is therefore apparent that oxalic acid, as deposited in crystals from solution, is decomposed at a considerably lower temperature than when it contains only one equivalent of water.

My observation on the decomposition of oxalic acid by sulphuric acid induces me to dissent from the explanation which M. Gay-Lussac has suggested. Sublimed oxalic acid, acted on by a considerable quantity of strong sulphuric acid, began to effervesce by being kept for a few minutes in a vessel of boiling water. The action indeed was slow, but it was perfectly distinct and continuous, the disengaged gas consisting as usual of equal measures of carbonic oxide and carbonic acid. The effervescence was much more free at 220°, and still more so at 230°; and yet the same acid, exposed to mere heat, would not have yielded a particle of gas at the temperature of 330°. Oxalic acid, in its most hydrated state, if mixed with a large excess of sulphuric acid, is decomposed at nearly the same temperature as it is after having lost two-thirds of its water, effervescing with moderate freedom at 220°; whereas it may be heated by itself to 230° without decomposition. As the temperature in all these experiments was ascertained by the same thermometer, they are quite independent of any error in graduation. Such an error, which must be slight if it exist at all, cannot account for the comparative results obtained in my own experiments, though it may in part explain the discordance in the observations of M. Gay-Lussac and myself. It may surely therefore be inferred, that the decomposition is not due to the sole agency of heat; and we may, I apprehend, continue to explain the phænomenon on the principle which has been hitherto generally admitted.

I concur with M. Gay-Lussac respecting the composition of

of the gas evolved from fully hydrated oxalic acid when decomposed by heat. According to my observation, the ratio of carbonic oxide to carbonic acid is always nearly as five to six. A similar mixture is evolved from the acid after two-thirds of its water has been withdrawn, provided the heat be slowly applied; but when the decomposition is very rapid, I have generally obtained a still smaller proportion of carbonic oxide. On one occasion this gas did not exceed 31 per cent. The statement of M. Gay-Lussac relative to the appearance of formic acid, and the explanation which he gives of its production, appear to me perfectly correct.

XLIV. *On a new Register-Pyrometer, for Measuring the Expansions of Solids, and determining the higher Degrees of Temperature upon the common Thermometric Scale. By* J. Frederic Daniell, *Esq. F.R.S.*

[Concluded from p. 279.]

I SHALL now collect together the results of the preceding experiments, for the purpose of showing what conclusions may be derived from them with regard to the degrees of temperature which they indicate when referred to the common thermometric scale. I shall make the calculations first upon the supposition that equal amounts of expansion denote equal increments of temperature; and I shall thus be enabled to compare the present series with that which I formerly obtained with my first pyrometer, and to offer a few remarks upon the differences of the two.

I shall adopt the corrected temperature of 662° (350° centigrade) for the boiling point of mercury, as proposed by MM. Dulong and Petit; which agrees very closely with the amount employed in my first calculations, and which, deducting 62° for the mean temperature at which my experiments commenced, gives 600° for the interval for which the several expansions were determined.

The first column of the following Table refers to the number of the experiment; the second to the mark of the register and the bar which was employed; and the third to the amount of expansion in the same, occasioned by boiling mercury, or 600° of temperature upon Fahrenheit's scale. The fourth column exhibits the arc measured upon the scale; and the fifth the equivalent expansion. The sixth contains the corresponding temperature; the seventh records the state of the metal, which was the object of the experiment; and in the eighth I have recapitulated the corresponding results of my former Essay.

Table

TABLE X.

No. of Experiments.	Mark of Register and Bar.	Expansion for 600°.	Arc measured on Scale.	Expansion.	Temperature.		Metals observed.	Temperature by former Pyrometer.
7	B Platinum	·0152	5° 49′	·0508	2005+65	2070°	Copper, fusing point.	2548°
8	II Platinum	·0159	6 10	·0537	2026+65	2091	Gold, fusing point.	2590
9	III Iron	·0229	9 2	·0787	2061+65	2126	ditto ditto	
12	IV Platinum	·0116	4 10	·0363	1877+65	1942	Silver, fusing point.	2233
13	V Iron.	·0203	7 24	·0645	1906+65	1971	ditto ditto	
14	I Platinum	·0116	6 16	·0546	2824+65	2889	Iron, fusing point.	3479
17	A Iron	·0203	2 45	·0239	708+65	773	Zinc, fusing point.	648
18	B Iron	·0245	4 7	·0358	876+65	941	Zinc, inflaming.	

The most remarkable fact displayed by the preceding comparison is the beautiful accordance of the results obtained from two metals whose expansions are so different as those of platinum and iron. The temperature indicated by the latter exceeds that by the former in the instance of the fusing point of gold 35°, and in that of silver only 29°; and this excess is in accordance with the conclusion of MM. Dulong and Petit, exhibited in Table II., that the expansion of iron increases in the higher degrees in a greater proportion than that of platinum.

The discrepancy between the temperatures derived from the observations with my first pyrometer and the present are considerable, but may be sufficiently accounted for by the differences in the circumstances of the experiments, without imputing inaccuracy to the instrument. In the paper to which I have before alluded, I stated that "I did not offer the results as positive and accurate determinations of the different degrees, but only as nearer approximations than any that had yet been furnished from actual observation. The only method which I had it in my power to adopt for the purpose, I do not consider to be susceptible of absolute accuracy. The arrangement made consisted of a muffle of black-lead placed in an excellent draught-furnace. This muffle was furnished with a door, through a round hole in which the stem of the pyrometer was passed up to the shoulder. Another door, which could be stopped at pleasure, admitted a full view of the interior.

terior. The metal to be tried was placed in a small black-lead receptacle, of the same thickness as the pyrometer tube, in the middle of the muffle. Now it is evident that the pyrometer so situated would indicate the mean heat of the whole of the muffle; which heat might, and did, vary in different parts. Of two pieces of silver of the same size placed within an inch of each other, one fused some time before the other." I also suggested that "means might be contrived to surround the instrument with the metal in a state of fusion; but that it required particular opportunities, which it was to be hoped that those would avail themselves of who had them in their power."

That the latter method is the only one which can admit of accuracy will be evident from a few reflections. Setting aside the inequality of the heat of different parts of the same heated muffle, which however is a consideration of the utmost importance, it is obvious that its temperature must considerably exceed the true melting point of the metal exposed to its influence. Just as a piece of ice would never melt in a chamber of the temperature of 32°, but would require a considerably higher heat in proportion to its mass to supply the caloric which becomes latent during the process,—a mass of iron would exhibit but little signs of liquidity till subjected to a heat much above its true point of fusion. When once in a liquid state, both would rapidly rise to the temperature of the medium to which they were exposed. When metals are melted for the purposes of the arts, they of course require to be heated very far beyond their fusing points, that they may flow into the minutest fissures [hollows] of the moulds in which they are cast, notwithstanding the cooling influences to which they are suddenly exposed. In some of the finer castings of brass, the perfection of the work depends upon the intensity to which the metal is heated, which in some cases is urged even beyond the melting point of iron. With a fire whose power in all cases must so greatly exceed the temperature required, it is necessary to bestow great care in supplying the metal gradually, as we have before described; as it is inconceivable with what rapidity it rises after the solid pieces are completely dissolved. Evidence of the same fact may be derived from the experiments of MM. Clement and Desormes, which I have before quoted. They calculated the heat of melted iron at 3988°, and of iron just on the point of melting at 3164°,—a difference of 800°. And it is clear from the circumstances of the experiment, that the former must have considerably exceeded the true melting point, or it never could have been transported in a liquid state from the crucible to the apparatus

ratus in which the water was heated or the ice melted. It is probable that the process which they employed, of the calorimeter, was not susceptible of great accuracy; but the discrepancy of the results from those which I obtained from the metal in analogous circumstances is not great.

Iron just melting	3164°	by the former
	2889	by pyrometer
	——	275° difference.
Iron melted at a high heat	3988	by the former
	3479	by pyrometer
	——	509° difference.

A similar excess also appears in their determination of the heat of melted copper, and obviously admits of the same explanation.

After performing these experiments upon the melting points of the metals, I was desirous of ascertaining the effects of the most intense heat which it was possible to produce in a furnace; and to measure the utmost limits of expansion in a platinum bar. For this purpose I made use of an excellent wind-furnace in the Royal Institution, in which upon former occasions hob-nails had been completely fused into a button.

Exp. 19.—The register I, which had not been the least injured by the previous experiments, was fitted with a new bar of platinum which had been drawn as a wire, was $\frac{5}{20}$ths of an inch in diameter and very ductile. The iron bar was also adjusted to a new register, and both were placed upright in a well luted crucible. About half an inch of powdered charcoal was strewed upon the bottom to prevent any adhesion; and two soft iron nails, and a piece of unglazed Wedgwood's porcelain, were thrown in for the purpose of affording some indication of the degree of heat attained. The crucible was then set in the furnace, another smaller crucible inverted upon it, covered with coke, and the heat urged to the utmost for two hours. The fire was suffered to burn out, and the crucible with its contents removed for examination. It was sound, but the luting had been completely fused. The nails were found melted into two complete buttons, and the porcelain was partially fused upon the surface.

The register I. appeared to be uninjured, but the platinum ring and wedge were loose, evidently from a contraction having taken place in the substance of the black-lead. This was no doubt owing to the heat having exceeded that at which it had been originally baked. The amount of expansion consequently could not be measured. The platinum ring, both of this and the other register, exhibited a remarkable change of texture; they had become very rough and crystalline, and

 were

were perfectly brittle, breaking easily between the fingers. The platinum bar also, which there was some difficulty in removing from the cavity, presented a very extraordinary appearance. It was apparently embossed with crystals, and was evidently larger at the lower end than at the top: it was also something contracted in length. Upon examination with a lens no regular facets could be detected, but it had the appearance of a bar constructed of plates of native platinum loosely welded together.

The register which contained the iron bar was considerably bent, and had several transverse clefts in its substance, owing possibly to its having become inclined in the crucible. Partial fusion had taken place upon the surface of the bar, which had run down and formed a knot at its lower extremity. About an inch of the same end was found to have been converted into steel, but all the rest retained the character of soft iron.

Exp. 20.—I repeated the last experiment with the same platinum bar in the register I. The arrangement was precisely the same, with the exception of the second register with the iron bar, and the fire was maintained with equal intensity for an equal time.

The iron nails were found perfectly melted, and the porcelain superficially fused as before. The ring and wedge, however, were fixed in their places, and the index undisturbed, but the measure was unfortunately lost from an accident. The texture of the platinum ring was changed, as in the previous experiment, and the bar tightly fixed in the cavity. By frequent gentle concussions it was removed without injury to the black-lead, which had some slight marks of fusion upon its surface, but was in a perfectly good condition. The bar was in a still rougher state than before, highly crystalline, and exhibited several large longitudinal clefts in its substance. It was found, by measurement with callipers, to be $\frac{1}{20}$th of an inch larger in diameter at its lower than at its upper end, and seemed to be approaching a state of complete disintegration. It was, however, perfectly hard and inflexible. My intention was to have again exposed it for several hours to the same degree of heat, with the expectation that the disintegration would have been complete, and that it would actually have fallen in pieces during the operation: in the mean time I chanced to make it red hot upon a common charcoal fire; and upon attempting to lay hold of it with a pair of tongs the two ends dropped off, and I only withdrew the small portion which I had grasped, and which was flattened and fractured by even this slight compression. The two ends were afterwards carefully, but with difficulty, raised from the fire, and when

when cold were perfectly hard and inflexible. I again heated a portion of the bar to a dull red, and it crumbled to powder from a slight blow with a hammer.

Exp. 21. — It being a point of the greatest interest to ascertain the maximum of expansion which took place in the platinum previous to this remarkable change of structure, I adjusted the original platinum bar, with which the greater part of my experiments had been made, and which presented a perfectly smooth surface, and was very soft and ductile, in the register I. A crucible was placed in the same wind-furnace, containing only a little charcoal powder, with the iron nail and fragments of porcelain as test pieces. The fire was urged to the utmost; and when it had been continued two hours the cover was removed, and the register, previously made red hot, was carefully introduced, the cover replaced, and the ignited fuel heaped upon it. At the expiration of a quarter of an hour it was lifted out and cautiously cooled. An excellent measure was obtained, and the arc determined to be $7° 24' =$ expansion ·0645.

The test pieces were found in the same state as in the previous experiments. The platinum bar was loose in the cavity, and had not altered its form; but its surface had assumed a slightly crystalline texture, and it had become very hard and inflexible.

The expansion registered would, upon the hypothesis before assumed of equal amounts of expansion denoting equal increments of temperature, indicate a heat of 3336°; or, adding the initial temperature 65°, = 3401°. But it must be remembered that this is probably rather the temperature at which the change in the structure of the platinum took place, than the utmost heat of the furnace. The latter may possibly exceed the degree at which the expansion of the metal ceases, and at which its particles evidently form a new arrangement; but this point cannot at present be determined. The coincidence of this result with that obtained in the former series of my experiments, is very remarkable. The temperature at which I obtained the fusion of cast iron at that time was calculated at 3479°, and was produced by the utmost energy of an excellent wind-furnace; and this, it will be observed, is within 80° of the present maximum.

Exp. 22.—Being desirous of ascertaining whether the register and platinum bar had undergone any change in their rates of expansion by the intense heat to which they had been exposed, I again adjusted the latter in the register I, which had now been once immersed in melted iron, and three times subjected to the action of the wind-furnace, and boiled them

 for

for ten minutes in mercury. The arc measured was 1° 19′ = expansion ·01148: the difference of 1′ may safely be ascribed to the uncertainty of the reading.

The temperatures thus determined will require correction, if we adopt the conclusion derived from the experiments of MM. Dulong and Petit,—that the dilatability of solids, referred to an air-thermometer, increases with the heat. The amount of this correction will be as the rate of increase; and according to those gentlemen is 11°·6 of the centigrade thermometer, or 20°·8 Fahr. from 32° to 572°, or the calculated temperature is to the true as ·00091827 : ·00088420. Supposing the increase of dilatability to continue the same for equal intervals of temperature, which however has not yet been proved, the following Table will exhibit the corrected temperatures derived from the preceding experiments with the platinum bar.

Table XI.

		Observed.		Corrected.
Melting point of silver		1942	...	1873*
————	copper	2070	...	1996
————	gold	2091	...	2016
————	iron	2889	...	2786
Temperature of the maximum of expansion of...	platinum...	3401	...	3280

If we reason in the same way from the increase of the dilatation of iron, as laid down by the same authors, the discrepancy between the temperature derived from the platinum and iron is very considerable; the melting point of silver coming out 1682°, and that of gold 1815° by the latter: but I conceive that the determination of this point in the iron is open to objections which do not apply to the platinum, and my suspicion is confirmed by the anomalous expansion of the iron exhibited in Tables V. and IX., and to which I shall recur upon a future occasion.

The general utility of the pyrometer, however, will in no way be affected by any uncertainty in these corrections. The indications which it is capable of affording will always be positive determinations, which it will be easy to modify by calculation, as our theories may improve. For all common purposes (and I must own that I look forward with hope that this instrument will prove eminently useful in many of the common processes of the arts) it will not even be necessary to note the expansion indicated by the arc measured; but each minute of the degree may at once be valued in degrees of

* Mr. Prinsep, from a laborious series of experiments upon the expansion of air confined in a bulb of gold, determines the melting point of silver to be 1830°.—Phil. Trans. 1828. p. 94.

Fahrenheit's

Fahrenheit's scale at the time of taking its rate of expansion by the boiling of mercury: and a Table of such values should be furnished for each register by the maker of the instrument. The following, for example, would be the proper Tab.ef or register I, which has been so often referred to, in which the arc for the boiling of mercury or 600° (without adding the initial temperature) was 1° 20′.

TABLE XII.

°	′		Expansion.		Temperature.	°	′		Expansion.		Temperature.
1	0	=	·00872	=	450	0	10	=	·00145	=	75
0	30	=	·00436	=	225	0	5	=	·00072	=	37
0	20	=	·00290	=	150	0	2	=	·00029	=	15
0	15	=	·00218	=	112	0	1	=	·00014	=	7·5

With such a Table an intelligent workman could employ the instrument without any material error. Those who might object to the expense of a platinum bar may substitute an iron one for ordinary purposes, and the cost of the black-lead register can never be an obstacle to its general use. Other substances might obviously be employed in its construction, but the facility with which it can be worked, its small expansion, its infusibility, and the impunity with which it bears the most sudden changes of temperature (as when red hot it may even be quenched in water without injury), will probably always give the black-lead ware the preference. The only precaution to be taken with it is to expose it previously, out of the contact of air, to a heat at least as great as that in which it is intended to employ the instrument.

XLV. *Correction of a Quotation in a Paper* "On the Impediments to the Study of Natural History," *published in the Phil. Mag. and Annals for May*, 1831. *By* W. S. MACLEAY, *Esq., M.A. F.L.S. &c.*

To Richard Taylor, Esq. &c. &c.

Dear Sir,

YOUR Correspondent who signs himself "*Ruralis*," has in the Phil. Mag. and Annals for May last given us a melancholy picture of "*the Impediments to the Study of Natural History.*" In his list of them, however, I think he has omitted to make mention of that impediment which I fear has most thwarted his own progress in the science. But this is of the slightest consequence; and indeed were it all, I would not have troubled you on the present occasion. But I must beg of your Correspondent that in future, when he does me the honour to quote from any work of mine, he will cite the exact words, and not make me the author of downright nonsense. I have nowhere said that "the discovery of a natural system, being the

the work of an all-wise, all-powerful Deity, can only be hoped for from a cautious process of inductive and analogical reasoning." And yet this stuff is put within commas, as if I had written it. Allow me to say also, in reply to the editorial note affixed to this singular quotation, that I have nowhere designated "any attempt to unravel" the Plan of the Creation, as the Natural System.

By the way of conclusion, I may remark that your Correspondent is happily not in any way obliged to trouble his brains with the "*repulsive and inscrutable*" innovations of "*revolutionary Zoologists.*" And therefore advising and hoping that in future he will stick close to "*the familiar and popular Entomology*" which amused him "in those happy days long gone by," I remain, my dear Sir, yours &c.

W. S. MacLeay.

XLVI. *Notice of a Geological Survey of the Mines of Cornwall; with a Programme of an intended Arrangement of the leading Details of the Metalliferous Veins, &c.* By W. Jory Henwood, *F.G.S.*

To Richard Taylor, Esq. &c. &c.

Dear Sir,

IN the year 1829, at the desire of R. W. Fox, Esq. of Falmouth, and C. Fox, Esq. of this place, who also defrayed the expenses incidental thereon, I made an examination of the leading geological features of several of the mines in Cornwall; and a short residence in the neighbourhood of Tavistock, in the summer of 1830, afforded me an opportunity of extending my investigations to some of the mines there. The results of these inquiries were communicated to the Royal Geological Society of Cornwall, in October last, and were to have been honoured with a place in the forth-coming fourth volume of their Transactions.

But in consequence of the liberal aid and encouragement of most of the noblemen and gentlemen resident in, and connected with, this county, as well as of several distinguished scientific men elsewhere, I have commenced a survey of all the mines in Cornwall; and I have now examined all those west of Redruth. Besides noticing the geological relations of the veins, ("lodes", "cross courses," "flucans," "slides," and "elvan courses",) and of the rocks they traverse, I ascertain the temperature of every stream I find running or jetting out of the *unbroken* rock, and make experiments on the electro-magnetic properties of the veins*.

After having gone through *all* the mines, I shall endeavour to arrange and classify my observations; and the Council of

* Fox, Phil. Trans. 1830, part ii.

the

the Cornwall Geological Society have kindly permitted me to withdraw the before-mentioned paper, in order to its being incorporated with the rest. When completed, I intend leaving my work at the disposal of the noblemen and gentlemen whose liberal assistance has mainly contributed to its completion. But before I proceed, I am anxious to submit to your geological readers a Programme of the manner in which I had intended arranging the details, and I shall be exceedingly obliged to any of them who will trouble themselves to point out the defects, and to suggest improvements. Those who are unacquainted with the interior of a mine will doubtless notice in this outline many points of interest not described; the contact of veins at various depths, unseen; and a general scantiness of information. I may observe, that the excavations are not always extended to the points at which interesting phænomena may be expected; operations may have been suspended, and the sides of the excavations either fallen together, been filled with water, or with foul air which extinguishes candles; the vein may have been entirely worked away, and in most cases the sides are after a short period invested with a crust of ferruginous matter, which rapidly accumulates. In a few instances I have not deemed it prudent to expose myself to falling rubbish, on decayed wood, or on rotten ladders. None but those who have experienced the like can fully feel the many vexatious disappointments I have had to suffer, in meeting an immovable barrier sometimes but a few fathoms, or even a few feet, from my object; after having crept hundreds of fathoms on all-four, and often on the belly, through mud and water, often beneath tottering rocks. These inconveniences, to which geologists who confine themselves to the neighbourhood of the surface are not exposed, have been every-day occurrences with me; and I venture to hope that they may with some propriety be offered as an apology for the deficiencies which my outline will at every step present.

I remain, &c.

Perran Wharf, near Truro,
August 11, 1831.

WM. J. HENWOOD.

PROGRAMME.

Cookskitchen Mine, parish of Illogan, near Redruth, is situated on the northern side of the granitic range, of which the nearest hill (Carnarthen Cairn) is only about half a mile distant, and its acclivity is at an angle of about 7°. The phænomena of the interlying of granite and slate which here occur have been often referred to, having for a considerable time been supposed to be peculiar to this mine. The eastern part of its surface is nearly horizontal; but towards the west it rapidly declines.

Lode.	Direction in		Dip in		Depth and Composition or Contents.
	Slate.	Granite.	Slate.	Granite.	
Small western Cross course	....	8° W. of N.	....	W. 76°	119 f^{ms}. Veins of radiated quartz, with friable granite interposed..........
Cross vein	....	about N.	....	W. 85°	119 f^{ms}. Two veins of radiated quartz, with a vein of friable granite between them .
Little Cross course	35° E	of N.	W. 67°	....	33 f^{ms} (In slate.) Angular masses of slate, with a vein of radiated quartz on E. side
				W. 80°	87 f^{ms}. Friable granite mixed with radiated quartz; a vein of granitic clay on W. side
South lode	....	....	....	....	Surface to 10 fathoms ....
					10 fathoms to 40..................
					40 fathoms to 49..................
					49 fathoms ....
	2° N.	of W.	S. 70°.	......	54 f^{ms}. Brown iron ochre, slate, quartz, iron, and yellow copper pyrites, "walls" smooth ....
				S. 60°	95 f^{ms}. Quartz and slate with a little copper pyrites ....
					112 f^{ms}. ditto ("walls" smooth)
				S. 72°	119 f^{ms}. W. of Cross vein, *yellow* sulphuret of copper ....
					E. of ditto *gray* sulphuret of copper ....
					E. of West Cross course, yellow sulphuret of copper with chlorite and friable granite .....
Toy's lode	5° S.	of W.	N. 66°	....	54 f^{ms}. Brown iron ore, quartz, and yellow copper pyrites ....
				S. 85°	72 f^{ms}. Friable granite and iron pyrites ..
					103 f^{ms}. Red iron ore and crystalline quartz, a vein of granitic clay on N. side
				N. 85°	112 f^{ms}. Red iron ore and quartz......
Great lode.....	10° S.	of W.	S. 65°	....	33 f^{ms}. Gray sulphuret of copper, brown iron ochre, slate and quartz; a vein of slaty clay........................
					67 fathoms ....
				S. 80°	112 f^{ms}. Gray and black sulphurets, and red oxide of copper, red iron ore, and iron pyrites. At the contact of Toy's lode, a vein of granitic clay ....
Dunstan's lode*	....	5° S. of W.	....	N. 60°	105 f^{ms}. Friable granite ....
Middle Engine lode*......	18° N.	of W.	N. 74°	....	33 f^{ms}. Quartz, chlorite, and yellow copper pyrites, two veins..............
					41 f^{ms} ....
				N. 63°	87 f^{ms}. Friable granite and gray sulphuret of copper ....
Hardshaft lode*	18° S.	of W.	N. 58°	....	31 f^{ms}. Slate, crystalline quartz and yellow copper pyrites ....
			N.	42°	54 f^{ms}. Slate, a vein of slaty clay on N. side
Sawpit lode ...	2°N. of W.	....	perpendi	cular ...	54 f^{ms}. Slate, red iron ore, and felspar clay
Lode N. of Sawpit lode	4°N. of W.	....	N. 68°	....	54 f^{ms}. Slate, brown iron ochre and iron pyrites ....

* The out-crop of these "*lodes*" (if there be any) is not seen.

Breadth or Size.	Dip of Stratification or Cleavage of Slate.	Appearance of Granite, and its Dip at Junction, &c.&c.	South Lode. Direction of Heave, and if to the greater or smaller Angle.	South Lode. Angle included.	Great Lode. Direction of Heave, and if to the greater or smaller Angle.	Great Lode. Angle included.
14 in.	....	....	R 3 f^ms GA	77°		
8 in.	....	....	R 7 ft. GA	85°		
4 ft.	Deep blue, dip N.W. 34°	At this spot there is also granite, which is traversed by many veins of a fine-grained dark gray schorl rock.	....	....	R 3½ f^ms SA	39°
1½ ft.	....	Near the cross course soft, and mica more abundant than at a little distance....				
......	....	Coarse-grained, soft, and tinged red: dip of junction S. 22°.				
......	Deep blue, dip N. 50°. 65°					
......	....	Fine-grained with imbedded crystals of schorl.				
......	N. side slate, dip N.W. 34° dip S. 12°	S. side of vein granite.				
2 ft.						
4 ft.	....	The slate and granite *gradually* pass off from one into the other, there being no distinct separation.				
4 ft.						
3 ft.	....	Traversed by numerous veins of the fine-grained schorl rock.	The "lodes" continue together for several f^ms on the line of the S. lode's dip; at the contact they present a confused mass. GA.			
3½ ft.						
5 ft.						
4½ ft.	....	....				
4 in.						
4 ft.	....	....	....		The lodes unite and continue in contact for 9 f^ms down; size 10 ft.; dip that of Great lode; contents red iron ore and gray sulph^t. of copper; no intersection discernable. GA.	
4 ft.						
3 ft.						
......	....	Dip N. 62°.				
5 ft.	Deep blue soft N. side dip N. 34°; and S.W. 52°	Spots of granite in the slate here.				
1 ft.						
8 in.						
......	....	Structure not uniform: nests of crystals of felspar & mica				
4 ft.						
2½ ft.						
4 ft.	Deep blue, N. side	S. side, much mixed with slaty particles.				
20 in.	Traversed by veins of felspar clay.					
3 ft.						

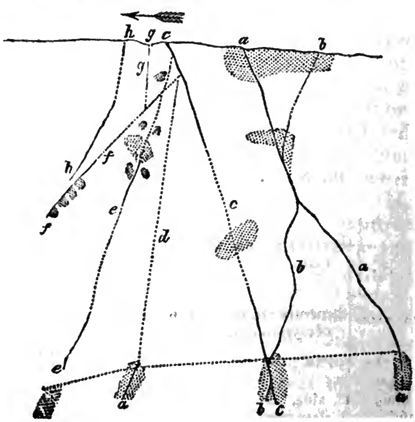
N.B. In the MS. two columns are added, giving the corresponding quantities for the Mid. Engine Lode. These are omitted for want of space. The only entries being for the "Little Cross Course," "W. 80°," and are "R 2¼ f^ms SA. 73°."—Edit.

In the preceding columns the "direction" is with regard to *true* north, and the dip in deviation from the *horizon.* When in its horizontal course one vein or *lode* is intersected by another, and the parts on opposite sides of the traversing vein are not exactly opposite to one another, the intersected "*lode*" is said to be "*heaved.*" Whether we approach the traversing vein from the east or west, on the course of that which it has "heaved," still in either case the dislocated portion on the opposite side will be found by turning to the same hand; and thus a heave to the "right" or to the "left" is the same on either side:—this is the meaning of the letter R in the foregoing Table. The GA and SA refer to whether the dislocated portion is to be found on the side of the "greater" or "smaller angle." The prevailing dip of the "bunches" of ore in Cookskitchen is to the westward; but I forbear entering on that branch of the subject, or on a discussion of the laws which may perhaps be ultimately found to regulate the distribution of metalliferous minerals, as such inquiries would be foreign to the object of this communication.

Cookskitchen Mine, Parish of Illogan, Cornwall: transverse Section.

[Scale 32 fathoms to an inch.]

Allusions to the accompanying figure. The shaded portions denote granite; the unshaded, slate. *a* South lode; *b* Toy's



Henwood del.

lode; *c* Great lode; *d* Dunstan's lode; *e* Middle Engine lode; *f* Hardshaft

f Hardshaft lode; *g* Sawpit lode; *h* Lode north of Sawpit lode. It seems probable that Hardshaft, Middle Engine, and Dunstan's lodes are only "*branches*" of the Great lode. The dotted lines respectively denote the suspected continuation of the various "lodes" and of the masses of granite;—these points are said to have been seen by others. The full lines denote what I have seen, the dotted portions what I have not seen.

XLVII. *Experiments on the Disinfecting Powers of increased Temperatures, with a view to the Suggestion of a Substitute for Quarantine.* By William Henry, *M.D. F.R.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen, Manchester, Oct. 14, 1831.

SEVERAL years have elapsed since I was requested, by an eminent merchant of this town* extensively concerned in the importation of Egyptian cotton, to take into consideration, whether any effectual method could be devised of guarding against the introduction of the Plague into this country by means of that raw material, without incurring the serious commercial sacrifices, which then attended the enforcement of the quarantine laws on large cargoes of that article. Chlorine might have been proposed for the purpose; but it was evidently inapplicable, not only on account of its chemical activity on vegetable substances, but of the necessity of washing and drying the cotton, in order to free it from any adhering portions of that powerful agent, the smallest remains of which would be injurious to the spinning machinery. In proposing any new method of destroying contagious matter, it was represented to me as quite essential that it should be incapable of impairing, by its chemical action, the tenacity of the fibre, as this would unfit the raw material for the operations through which it has subsequently to pass.

By this restriction, the ground for experiment was considerably narrowed; and after giving much attention to the subject, no means occurred to me of effecting the object in view, but that of applying to the raw cotton such a degree of heat as, while it should do no injury to the staple of the article, might yet be sufficient for the destruction of any contagious virus which it might have imbibed.

That the contagion of the plague, supposing it to be present in the state of *fomites*†, might be rendered innoxious by a temperature

* William Garnett, Esq.

† *Fomites* (the plural of *fomes*, fuel) expresses contagious or infectious matter

temperature below that of boiling water, appeared to me not improbable, from the evidence of a fact recorded by various writers; viz. that the plague, in countries where it prevails, ceases as soon as the weather becomes very hot. "Extreme heat," says Dr. Russell in his Natural History of Aleppo, (vol. ii. p. 339.) "seems to check the progress of the distemper; for though the contagion and the mortality increased during the first heats in the beginning of the summer, a few days continuance of the hot weather diminished the number of new infections. July is a hotter month than June; and the season, wherein the plague always ceases at Aleppo, is that in which the heats are most excessive." In another part (p. 284.) of the same volume, Dr. Russell states the greatest heat at Aleppo in June to have been 96° of Fahrenheit, and that of July 101°, in the shade.

Arguments, also, derived from chemical reasoning, appeared to me to strengthen the probability that a temperature, raised to no great extent, would suffice for the decomposition of infectious or contagious matter*. Of the nature of contagion we are, it is true, entirely ignorant. But we are entitled to conclude that it is in no case identical with any one of the simple or compound gases, with which chemistry has made us acquainted, and which are unchanged by a temperature below 212°; because each of those gases has been breathed, many of them very frequently, without exciting a specific disease. The subtile poisons which propagate contagious distempers, being the products of organic life and of morbid conditions of the animal body, are, it is probable, of a complex nature, and owe their existence to affinities which are nicely balanced and easily disturbed; even more easily than those maintaining some of the products of vegetable life, which lose their original properties, and acquire new ones, when exposed to temperatures of no great amount. Thus starch is converted by a moderate heat into a substance somewhat resembling gum; and, by weak chemical agents, into sugar. Among inorganic compounds, we have a remarkable instance of the effects of heat (raised however to a higher degree) in the change of *phosphoric* into *pyro-phosphoric acid*. In most cases of this kind, it is pro-

matter existing in absorbent substances, such as wool, clothing, &c. In this state of confinement it seems to acquire increased virulence and activity.

* I use the term 'infection' and 'contagion' as synonymous, because no sufficient distinction has been established between them. It would be unseasonable to enter, in this place, into a disquisition about words; but those who take an interest in the verbal part of the subject, will find an excellent view of it (pointed out to me by my son Dr. Charles Henry) in the *Dictionnaire de Médecine*, art. *Contagion*, vol. v. p. 549. Paris 1822.

bable

bable that increased heat produces no change either in the *number* or *proportions* of the atoms of the substances; but that, in some way which chemical science has not yet prepared us to explain, it modifies only the *arrangement* of the atoms, and thus confers new distinctive characters.

In pursuing the inquiry experimentally, two circumstances seemed to me to require to be established.

1st, That raw cotton, and other substances likely to harbour contagion, would sustain no injury by the temperature conceived to be necessary for their disinfection.

2ndly, That in at least some one unequivocal instance, contagious or infectious matter should be proved, by actual experiment, to be destructible at that temperature.

I. To ascertain the first point, I submitted in August 1824, a quantity of raw cotton to a dry temperature of 190° Fahrenheit, which was steadily kept up in the inner compartment of a double vessel heated by steam, during two hours. When the staple of the cotton was examined by Mr. Garnett, he pronounced it to be so essentially injured, as to set at rest, on a first view, all intentions of adopting this method of purification. The same unpromising appearance was presented also by cotton yarn, which, after being spun, had been heated during two hours at 190° Fahrenheit. After being allowed to cool during a quarter of an hour, it was compared with yarn of the same fineness which had not been heated, with the following result:

	lbs. avoird.
A hank of mule twist (40 to the pound) not heated, required a weight, to break it, of..................	$246\frac{1}{4}$
A hank of ditto ditto heated to 190° and just cooled	$166\frac{2}{3}$

The strength of the yarn, measured by its power of supporting weights, had therefore suffered a diminution, by being heated, of fully one-third. The remainder of the yarn so heated having been laid aside in a cellar, was accidentally examined on the fourth day, and had undergone an obvious alteration, which led to a renewed trial of its strength. It was now found that a hank of the same yarn supported a weight of $241\frac{1}{4}$ pounds, and it had therefore recovered very nearly its original tenacity.

At this period I was obliged, by unavoidable circumstances, to abandon the inquiry; and the inducement to resume it ceased in a great measure to exist, in consequence of the discontinuance, for a season, of the pressing inconvenience which had given birth to it. It was only recently that my attention was recalled to the subject by the well grounded alarm

alarm which has overspread the continent of Europe, and in a less degree extended over this country, in consequence of the devastating effects of a disease (*cholera*), the contagious nature of which is rendered highly probable, and which, like other contagious diseases, may be presumed to be capable of being conveyed by *fomites*. It is therefore of the greatest importance to devise effectual and easily practicable means of extinguishing the first sparks of that distemper which may show themselves in this country, avoiding at the same time greater injury than is necessary to individual interests or to general commercial prosperity.

The first step which appeared to me desirable, on resuming the investigation, was to decide, beyond all doubt, whether raw materials, as well as manufactured goods and articles of clothing, could be exposed without injury to a dry heat approaching 212°. Of raw materials, I took cotton as the one which, from local advantages, I could best submit to the necessary trials; and I had the benefit of the zealous assistance of a friend* engaged in the spinning branch of that manufacture. Raw cotton of ordinary dryness, as recently taken from the bag, was exposed, during two or three hours, to a steady temperature of 180° Fahrenheit, in a vessel heated by steam of common density. It lost, generally, between two and three ounces from the pound. The effect on the staple, as determined by the inspection of persons versed in the article, was apparently such a degree of injury, as to forbid all expectation that the cotton could be rendered useful. It was pronounced to be rotten, and what is technically called '*fuzzy*,' and to be unfit even for those operations which are preparatory to its being spun into yarn. After being left, however, during two or three days, in a room without fire, a great change had taken place in its appearance, and it was found on trial to be as capable of being spun into perfect yarn, as cotton employed in the ordinary manner. On an accurate trial of the twist which had been spun from it, a hank supported fully an equal weight, with a hank of the same fineness spun from cotton fresh from the bag. This fact, established by repeated experiments, proves that with the recovery of its hygrometrical moisture, cotton, which has been heated, regains its tenacity, and becomes as fit as ever for being applied to manufacturing purposes.

Articles of cotton, silk, and wool, after being manufactured, both separately and in a mixed state, into piece goods, for clothing, were next submitted to the same treatment.

* Peter Ewart, jun. Esq.

Among

Among them were several fabrics, which were purposely chosen, of the most fugitive colours and delicate textures. After being exposed three hours to a temperature of 180°, and then left a few hours in a room without fire, they were pronounced, by an excellent judge of the articles who furnished the specimens, to be perfectly uninjured in every respect. Furs and feathers, similarly heated, underwent no change; and there can be no doubt that if the apparatus had enabled me conveniently to have raised steam of increased density, a temperature above 212° Fahrenheit would have done no injury to the delicate and costly articles submitted to it.

II. The most important point to be ascertained, and that on which the utility of the inquiry hinges, is whether a temperature below 212° Fahrenheit is capable of destroying the contagion of *fomites*. The investigation is one of great nicety, and involves considerable difficulties. It was entirely out of my power to try the agency of heat on those contagions which propagate the formidable diseases of cholera, plague, scarlatina, typhus, &c. The only way, in which I could arrive at an analogical inference respecting the decomposing power of heat over such contagions, was by determining its effect on some kind of infectious matter which assumes a tangible form, and can in that form be submitted to experiments; and which admits also of being afterwards tested by justifiable trials on healthy persons. Nothing presented itself to me, on consideration, so well adapted to fulfil all these conditions, as the matter of cow-pock. On mentioning my views to Mr. Roberton, one of the surgeons of the Manchester Lying-in Hospital, he obligingly suppplied me with vaccine lymph, taken from pustules of unequivocal character; and after the lymph had been subjected to high temperatures, he directed the insertion of it to be made, in the usual way, into the arms of healthy children. The trials were conducted, and the results registered, by Mr. Gee, the House-Apothecary of the Hospital.

1. Vaccine lymph, dried at the temperature of the atmosphere on small bits of window-glass, was exposed to a heat of 180° Fahrenheit during four hours. Three healthy children of proper age were inoculated with this matter without any effect; but being afterwards vaccinated with fresh matter, they all took the disease.

2. Lymph heated, during the same period, at a temperature varying from 120° to 140°, generally 130°, was inserted without effect into the arms of two healthy children, who were afterwards successfully vaccinated with recent matter.

3. Four pieces of window-glass, on which recent vaccine lymph

lymph was placed, were heated during intervals varying from two to three hours, at a temperature never below 160° nor above 165° Fahrenheit. The trials were judiciously varied by Mr. Gee, by inserting each specimen of the matter which had been dried, into one arm only of a healthy child; while into the other arm of the same child recent matter was inserted. In every instance, the heated matter proved inefficient; while the matter which had been dried at the temperature of the atmosphere produced a satisfactory pustule.

4. For the sake of obtaining a sufficient number of instances, I requested Mr. Marsden, House-Surgeon of the Manchester Royal Infirmary, to make trial of some genuine vaccine lymph which I had received from him, and had then submitted to heat. One specimen had been placed two hours in a steady temperature of 150°; a second four hours in the same temperature; a third two hours, and the fourth four hours, in the temperature of 172°. In no one instance, did any of these specimens, when inserted in the usual manner, produce the vaccine pustule.

5. Descending in the scale of temperature, another portion of vaccine lymph was exposed to an uniform heat of only 120° Fahrenheit for three hours. Two children, inoculated by Mr. Gee with this matter, received the infection, and the pustules were, in each case, remarkably well characterized. From their arms matter was taken, with which upwards of forty children have been vaccinated, who have gone through the disease in the most satisfactory manner.

It may be considered, then, as established by the experiments which have been related,—1st, That vaccine matter is not destroyed by a temperature of 120° Fahrenheit; and it is even probable that it would sustain, without losing its efficiency, a heat several degrees higher;—2ndly, That vaccine lymph is rendered totally inert by exposure to a temperature of 140° Fahrenheit. May we not hence infer, that those subtile animal poisons which lie dormant in the state of *fomites*, are likely to be disarmed of their terrors by the same simple means? The expectation, I am aware, rests entirely on analogy; but the analogy appears to me sufficiently strong to render it desirable that it should become the parent of experiments. It is with that view only that I propose it to the enlightened physicians of this and other countries, who have the means of verifying or disproving the inference by experiments on the more diffusible and active contagions. Until, indeed, the soundness of the analogy has been established by a sufficient number of facts of the latter class, no extensive practical measures can safely be grounded upon it.

If

If a favourable result should however issue from these suggestions, nothing can be more easy or less expensive in construction, or more manageable in use, than an apparatus for subjecting articles imported from unclean places, in any quantity, however large, to the disinfecting agency of a dry heat, without even the slightest injury to the quality of those substances. A double vessel, made of copper, or of tinned or cast iron, of any convenient shape, with a sufficient space between the two vessels for containing steam, and an interior cavity of due size for a receptacle of the articles to be disinfected, is the essential part of the arrangement. To avoid all risk of the escape of any portion of the virus in an undecomposed, and therefore active, state, a pipe, open at each extremity, may be carried from the receptacle into the flue of the chimney, or, better still, into the fire-place under the boiler, which will ensure the destruction of the contagious effluvia. The articles should be introduced into the receptacle, not closely packed, but so opened out, that every part of them may be exposed to the necessary temperature. If injury should be apprehended from over-drying any substance, a small quantity of steam may be suffered to pass through a pipe from the boiler into the receptacle. At every sea-port to which ships are bound with unclean bills of health, an apparatus of this kind should be provided, on a scale sufficient for the emergency. And on the Continent, similar provision should be made, at every barrier which is destined to prevent the introduction of contagious diseases.

It must be obvious that these precautions offer no security against the danger of a contagious disease breaking out in a person who has already been exposed to infection, but in whom symptoms of the disease have not yet manifested themselves. Risks from this source constitute, however, a very small proportion, compared with those arising from *fomites;* and they may be easily and effectually guarded against, by insulating the person supposed to be infected, for a period of time exceeding that, during which the seeds of the disease have been ascertained to lie dormant in the animal system. Nor is this proposal meant to supersede the employment of chemical disinfectants, especially of preparations of chlorine, in the apartments of the sick; or their application to articles and fabrics which sustain no injury by exposure to those agents.

I am, Gentlemen, your obedient servant,

William Henry.

XLVIII. *Notices respecting New Books.*

Montagu's Ornithological Dictionary; new edition, "with a Plan of Study, and many new Articles and original Observations. By James Rennie, *A.M., A.L.S., Professor of Natural History, King's College, London; Author of 'Insect Architecture,' 'Insect Transformations,' 'Architecture of Birds,' &c."* London, 1831. Octavo; Introduction, &c. pp. lx., Dictionary and Index, pp. 592; 28 Engravings on Wood.

"On my plan, any person, with a little care, may become a tolerably good naturalist, the first walk he takes in the fields, without much knowledge of books."—*Mr. Rennie's Introduction*, p. iv.

THE Editor of this new edition of Montagu's Ornitho logical Dictionary, and author of the introductory matter now prefixed to that work, has recently been appointed to the chair of Natural History in the King's College of London, (that is, we suppose, to the chair of Zoology, since Mr. Burnett is Professor of Botany.) Having been known to science previously only by the compilations (interspersed with some observations of his own), of which he is stated to be the author in the title-page quoted above, and which form part of the Library of Entertaining Knowledge, he has undertaken the publication now before us, we presume, for the purpose of evincing his fitness for the duties, as Professor of Zoology in one of our new national establishments for scientific education, which have been committed to his charge. He has also still more recently announced, in pursuance, we presume, of the same purpose, his intention of publishing "A Conspectus of Butterflies and Moths," and a translation "with copious notes and synonymes" of Le Vaillant's "Birds of Africa," "Birds of Paradise," and "Parrots."

We proceed, therefore, to examine the claims to regard as a man of science and as a public teacher of zoology, which Mr. Rennie has asserted in the volume now under our consideration.

The introductory matter is arranged under the following heads: "Introduction,"—"Plan of Study,"—"The Use of System,"—"System of Linnæus and Latham,"—"The Quinary System and Modern Doctrine of Types, Affinities, and Analogies,—and "Catalogue of Naturalists," subdivided into "Rudimental Naturalists,"—"Literary Naturalists,"—and "Philosophic Naturalists, and Original Observers."

On perusing this introductory mattter from p. iii. to p. lx., we were struck with the extreme assumption and arrogance of the whole style of treating his subject, which is here displayed by the author; with the bitterness and contempt of his vituperation of the naturalists whose views he condemns, disingenuously mingled with praise, which on his own showing must be undeserved; and with the perverse ignorance from which alone such misrepresentations as he makes on all the subjects which he touches, could have arisen. We affirm, *in limine*, that his statements respecting the Quinary System and every subject connected with it, are a tissue of errors, from beginning to end. He does

does not *understand* any of the subjects he has undertaken to discuss; he cites his own blunders as characters and attributes of the systems he impugns, and then actually takes credit to himself for overthrowing them. There is no such thing in the entire introduction as a fair *statement* or an *examination* of any of the systems he proposes to consider; all is arrogant, unsupported assertion, mingled with garbled extracts. His criticism of the various systems of ornithology or of zoology in general, is exceedingly confused and intricate, and difficultly intelligible; and many of his observations are utterly inapplicable, referring only, in reality, to his own misconceptions of the systems impugned.

Conscious, apparently, of the charges that would be preferred against him, on account of the misrepresentations which we have now briefly characterized, Mr. Rennie, in p. v. of his "Introduction," offers the following apologetical remark:

"The offer to print any reply to my arguments, which might be sent me, exculpates me, I conceive, from all charges of a personal nature; and it would grieve me much, if my dislike to their doctrines and language [those of Mr. W. S. Macleay and his disciples] has, in any instance, betrayed me to infringe upon the courtesy and decorum which ought uniformly to characterize such discussions. To enter into any compromise with error, would be unpardonable weakness and delinquency; but to endeavour, by contempt or abuse, to hurt the feelings of the person judged to be in error, would exhibit the character of a bully or a ruffian."

But we will tell Mr. Rennie, that the offer to print any reply to his arguments that might be sent him, affords no excuse whatever for making false representations, (and many such has he made,) which must necessarily have an effect upon the public mind unfavourable to the subjects of them, before the replies can appear. As well might the defamer of the character of any private individual hold himself exculpated from the charge of slander, by his offer to print a denial of his unjust representations, after they had gone forth to the world, to the injury of the object of his attack. It is difficult to conceive, also, that Mr. Rennie's professions of sorrow, if he should be found to have been betrayed into an infringement of the courtesy and decorum which ought to characterize scientific discussions, can be sincere, when we observe, in almost every page of his portion of this volume, the most palpable violations of courtesy, of decorum, and of truth. What his intentions may have been it is impossible for us absolutely to know, but it is certain that the pages before us present many examples of contempt and abuse, which in their own intrinsic quality, would become only those characters to which Mr. Rennie, in the concluding sentence of the above extract, has rightly ascribed the exhibition of them.

We now proceed to prefer against Mr. Rennie, *seriatim*, the charges which, as it appears to us, he has justly incurred by the representations made in the introductory matter to his edition of Montagu's Ornithological Dictionary now before us. We pledge ourselves to prove these charges, by the most ample and satisfactory evidence, as we proceed in this review. We shall intersperse them now with various facts re-

specting Mr. Rennie's character as a naturalist, which the perusal of the introductory matter discloses. His incapacity for the office he has assumed of Editor of Montagu's work, will be made abundantly evident hereafter.

Mr. Rennie appears to be wholly ignorant of the higher philosophy of Natural History, considered either as a branch of science, as a means of training the mind to the love of truth, or as an instrument of leading it to the admiration and adoration of the Creator. He seems to have little knowledge or perception of the vastness of nature, and none whatever of the fact, that there are such things as *design* and *order* in the distribution of the beings and substances which compose it.

He arrogantly misrepresents the scientific character of Linnæus, most disingenuously adopting the remarks upon it made by Mr. W. S. Macleay and some of the naturalists of the School he has founded, which have been suggested to them by their peculiar views; views upon which he afterwards heaps abuse, and still more deeply misrepresents.

His opinions of the merits of the various investigators of nature whom he has occasion to mention, of the highest rank in their respective departments of science, are pronounced in the most conceited and unbecoming manner: the most eminent and learned naturalists, whether practical observers or systematists, are equally the objects of his contempt: thus we have "the dry, lifeless, marrowless, and unphilosophic descriptions of the Linnæan school" (p. xxv.); their "gross inaccuracy" (p. xxx.); the "Linnæan barrenness of idea and of deduction" (ib.); the "hot and testy" behaviour of Linnæus (p. xxxvii.); the "briefness and poverty" of Pennant (p. xxvi.); the "credulous absurdity" (ib.) and the "wild, mischievous, and most absurd analogies" (p. xlviii.) of Cuvier; and the "trash" of Mohs and Haidinger (p. xxvii.)*.

Mr. Rennie further betrays the most palpable want of knowledge of the ordinary meaning conveyed by common forms of expression. He mingles some portions of the nomenclature of the Quinary System, in an insidious manner, with the monstrous errors and absurdities

* In the Englishman's Magazine for August last is an article entitled "Mismanagement of the Library of the British Museum," which we have been informed is from the pen of Mr. Rennie. Its style in every respect corroborates this information, exactly resembling that of the prefatory matter to Montagu. We mention it here, because Mr. W. S. Macleay's *Annulosa Javanica* is characterized in it (p. 589) as a "flimsy production," the "effrontery" of its "presumptuous author" being mentioned; while Dr. Horsfield's *Lepidopterous Insects* is stated to be a "worthy companion" to the *Annulosa Javanica*, being, therefore, also "a flimsy production" manifesting the "effrontery" of its "presumptuous author."—The flimsy productions, effrontery, and presumption of Mr. W. S. Macleay and of Dr. Horsfield! authors of some of the most splendid discoveries and profound researches in zoology which have ever been accomplished, and whose reputation is spread throughout the civilized world! To what part of the duties of Professor of Natural History in the King's College of London does this treatment of two of the most eminent cultivators of that science belong?

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which he has ignorantly confounded with that system, so as to induce the reader who is previously unacquainted with the subject, to conclude that Mr. Rennie's representations must needs be founded in truth, when he would have had nothing but Mr. Rennie's assertions to support that conclusion, had an ingenuous plan been pursued.

In the most false and unfounded manner he confounds the atheistical doctrine of appetencies, broached by Darwin, Lamarck, and Robinet, with the Macleayan doctrine of the progression in affinity from one group of animals to another, or the variation of form of each species from that possessed by the preceding one, so that on examining the entire group, a progressive change of character is observable, made up of the separate differences between each pair of contiguous species. He asserts, with equal violation of the truth, that the Quinary System, "while it professes to reject this strange doctrine, at the same time adopts its very language in the most unequivocal manner." (p. xxxiii.) The truth being, that the doctrines respecting natural distribution, held and advocated by Mr. Macleay, or more properly speaking the *phænomena* which he has discovered in the progression of affinities, &c. in natural history, afford the most triumphant refutation of the doctrine of appetencies; and that the entire scope of some of Mr. Macleay's arguments is directed against the very errors in zoology upon which that doctrine is founded.

To support these misrepresentations, Mr. Rennie affirms, with equal falsity, that Mr. Macleay has borrowed some of his general expressions from Robinet, and, as he would insinuate, with the intention of imparting the same ideas as that writer. (p. xxxv.) He further expressly asserts, with the same want of truth, that Mr. Macleay's "doctrine of types" is "directly borrowed from the atheistic system of Robinet." (p.xxxviii.) The Rev. W. Kirby, one of the authors of the celebrated "Introduction to Entomology," a work which is distinguished by the strain of rational piety and fervent devotion which it breathes, not less than by its accurate scientific details, is charged (p. xxxviii. &c.) with adopting (from Macleay) the atheism of Robinet! The extreme effrontery of this, (as we are sure all our readers will unite with us in regarding it,) becomes more strikingly apparent, when we reflect that the venerable naturalist thus accused was, not long since, selected to produce one of the works to be published in demonstration of the Divine Attributes, as manifested in the Works of the Creation, in pursuance of the bequest of the late Earl of Bridgwater. That this was a peculiarly appropriate selection, all who are acquainted with the works of Mr. Kirby, will concur in thinking; and what renders the most unfounded attack upon him in the publication before us the more extraordinary, is that it should proceed from a Professor in a College, two of the Governors of which (the Archbishop of Canterbury and the Bishop of London) concurred with the President of the Royal Society (then Mr. Davies Gilbert) in the appointment of Mr. Kirby to the above office *.—But to return to Mr. Rennie. Pursuing the same strain, Messrs. Macleay and Kirby are actually both charged, in the

* See Phil. Mag. and Annals, N.S. vol. ix. p. 202.

page

page last quoted, with assuming "that a stone has improved itself into an oak, and a horse into a man!" The idea of deviation in structure from a type previously discovered by an inductive process, or that of the assemblage of characters which belong to the species forming the *aberrant* groups of Mr. Macleay, is confounded in the work before us with the notion of absolute imperfection and degradation in the works of the Creator. The mental process by which we express the gradual change of form and structure observable in the progression of affinities among animals, is mistaken by Mr. Rennie for the actual physical conversion of one animal into another by the exercise of its own volition, and Mr. Macleay is charged with advocating the latter doctrine! The most philosophical and profound deductions of the most eminent naturalists of all ages, are also stigmatized by him as being nothing but the vagaries of fancy.

Finally, Mr. Rennie, rightly anticipating that he would be charged with misrepresenting the Macleayan System, and the opinions of its discoverer and advocates, attempts to excuse himself by confounding his own groundless inferences with the mistakes regarding the subject, of certain naturalists whom he names, but whom we will not degrade by naming unnecessarily in the same page with him.

We here terminate our indictment of Mr. Rennie at the bar of scientific and literary justice, for the numerous misrepresentations of fact, and misinterpretations of reasoning, of which the introductory matter of his edition of Montagu's Ornithological Dictionary is composed; and we proceed to state the process we have adopted in order to obtain the evidence necessary to support our charges, and the manner in which we intend to bring it forward.

Agreeably to Mr. Rennie's invitation in p. xxi. we have weighed "every fact" which he has adduced; we have "rigidly" scrutinized "every inference" he has made; and having found them wholly "wanting in truth and accuracy," "at once," as he calls upon us to do, "without any compromise," we "reject them." And, in accordance with our duty as conductors of a scientific Journal, we intend, by detailing the results of our weighing of facts and scrutiny of inferences, to evince that our readers also must, "without any compromise," "reject" Mr. Rennie and his works, as having any claim to the attention of the cultivators or students of science, or to that of the admirers of nature.

All the representations concerning Mr. Rennie's mode of treating the subjects he has undertaken to discuss, which are preferred in the foregoing pages, we engage to substantiate in detail, refuting at the same time such of his assertions as may appear to require it, in the course of the present article. In doing this, we shall have occasion to enter into an examination of certain errors respecting the "new views" in Natural History, which, as we conceive, have been committed by several contemporary naturalists; but we shall most carefully distinguish their candid criticism and expression of their sentiments from the arrogant and baseless assertions of Mr. Rennie, which involve the same fallacies of reasoning. We shall also endeavour, as we proceed, to explain and define, in their true characters, the

the views of natural arrangement of Mr. Macleay and his disciples, so far as they have yet been enunciated; noticing some differences of opinion, on minor points, which exist among the naturalists of this school. By this means we hope to convey a just idea of them to the general reader; for we wish to enlist every reflecting mind interested in the study of nature, in the support of the "new views," confident alike of the delight which the truths they unfold will convey to every person of common intellectual powers, and of the increased stability they will be found to give to the deductions of a sound natural theology, harmonizing most perfectly with the Christian Religion.

For reasons which will be evident in the sequel, we begin with Mr. Rennie's attack on the Quinary System. This commences in p. xxxii. of his prefatory matter, and in the following page, under the head "The Quinary System and Modern Doctrine of Types, Affinities, and Analogies," he ostensibly begins the consideration of the subject. Mr. W. S. Macleay's attempt to discover *the natural system* he states to have been "beyond all question, highly laudable," though, he continues, "I shall endeavour to show, after giving a brief outline [of it], it appears to be altogether a failure." Mr. R. then gives an extract from a review published in the Zoological Journal, and four extracts from Mr. W. S. Macleay's own works, for the purpose, apparently, of supporting a representation which he makes at the outset, that the "system recently proposed," to which he is about to advert, "on its first announcement, put forth the high claim of being exclusively,—if not *the natural system*, at least the rudiments thereof, or furnishing the means for arriving at this, and, therefore, [of being] in accordance with the plan of the Deity at the creation."

Now with this representation of the character of the doctrine of the circular succession of affinities and the parallelism of groups, (as we shall for the present designate the system under examination, for the particular number of the groups it discovers is merely a *consequence* of its other principles,) we heartily concur. To maintain that the arrangement discovered by Mr. Macleay is the *entire* system of nature, —that it embraces the *whole* plan of the Deity at the creation, or that errors may not exist in what Mr. Macleay or his followers may have promulgated as the result of their observation of nature, would be idle and absurd; and we confidently affirm that such a view never has been maintained either by Mr. Macleay or his disciples.

But we also affirm that the system first propounded, as a system, by Mr. Macleay, and *discovered* by him (however certain constituent principles of it might have been previously discovered by others) is as much "the natural system" as the Copernican system of that assemblage of the heavenly bodies of which the planet we inhabit is one, is "*the natural system*". Everything that proceeds from the mind or the hands of man, is, in the universal sense of the term, *artificial*; for what is *produced* by the exertion of the human mental faculties, or the human corporeal organization, cannot be *natural*, cannot be, *ipso facto*, what *exists in nature*. But when nature is observed by man, and when man expresses in language or by visible signs, his conception of what he has thus observed in nature, the logical or predicative system,

system, or assemblage of observed truths, so produced, is, in the language of science, *the natural system*. Thus what we call the "Copernican System" of nature, (truly, be it observed, a natural system,) is not the actual assemblage of planetary bodies circulating round the sun, of which our planet forms a part, but it is a *representation of it*, exactly corresponding to the truth; being *all* that man can know of the reality. We make these remarks in this place, because much needless misapprehension, and also much unprofitable discussion, have taken place on this part of the subject, and on the right use of the phrase "the natural system". The expression, by man, of the truths he has discovered respecting the system of nature, if that expression be itself true, is, in the just and legitimate sense of the term, "the natural system"; being all that a finite being can know or possess of it.

We further maintain that this is the sense, and the only sense, in which the phrase "the natural system", and other equivalent terms, have ever been used by Mr. Macleay and the naturalists of the modern British School of Zoology, of which he is the founder; and that it is the sense, and the only sense, of that phrase, and of the allusions to the same subject, as employed in the extracts cited by Mr. Rennie, in the page now before us.

In the extract from the review in the Zoological Journal, Mr. Macleay is characterized as "that profound zoologist who has succeeded more effectually than any of his predecessors in unravelling the intricacies of the system pursued by Nature in the distribution of the animal kingdom". What can be more explicit than this? the very identical system propounded by Mr. Macleay is not regarded as *being* "the system pursued by nature", but he is said to have been more successful than his predecessors in unravelling the intricacies of that system; just as we might say that Haüy was more successful than his predecessors in unravelling the intricacies of the system "pursued by nature" in the production of crystallized minerals; or that Mr. Dalton has been more successful than his predecessors in unravelling the intricacies of the system "pursued by nature" in the constitution of chemical combinations.

Mr. Rennie's statement insinuates, though it does not broadly affirm, that Mr. Macleay identifies *his* views with the actual system of nature, as existing in nature; now this we positively deny, and we deny too, that such a sense can be fairly or honestly extracted from the passages quoted. After having examined the preface to the *Horæ Entomologicæ*, from which Mr. Rennie's first two quotations are made, we affirm that no approach to such an identification is contained in it. The reader will observe that in these passages Mr. Macleay does not once mention his own views, but merely places in apposition "an artificial system" (understanding thereby any such system) and "the natural system"—"the plan of the creation itself—the work of an all-wise, all-powerful Deity". But he would suppose, from the connection in which the extracts are introduced by Mr. Rennie, that where "the natural system" is mentioned in them, Mr. Macleay means his own individual views. Than this, however, nothing can be

be further from the truth; Mr. Macleay does not, in the entire course of his preface, even once either expressly make or indirectly imply such an identification. He speaks throughout of the natural system as a thing which it is the end of the pursuit of natural history to discover, not as a thing yet discovered by any naturalist, and not at all as having been discovered by himself. Nor can the assumption Mr. Rennie would insinuate be discovered, (neither is it in the smallest degree implied,) in the quotations which follow from Mr. Macleay's paper in the Transactions of the Linnæan Society, and from his "Letter on the Dying Struggle of the Dichotomous System."

But previously to our entering in detail upon this part of the subject, we must, for another purpose, quote and make some remarks upon the remainder of this page of Mr. Rennie's book; it is as follows:— "Again, speaking of his discovery of what he calls the *nature* of the difference between affinity and analogy, Mr. Macleay says, 'It is quite inconceivable, that the utmost human ingenuity could make these two kinds of relation tally with each other, *had they not been so designed at the Creation*.' * In another place he talks of portions of his system being 'almost mathematically proved to be natural †.'" The *italics* are all Mr. Rennie's. We have in the extract now before us, the first example of a numerous class of misinterpretations of the plainest figures and forms of speech in the English language, which distinguish the present production of this champion, before whose mighty prowess all the discoveries of modern zoology are to be dispelled, like the illusions of fancy before the blaze of truth; a class of errors, which must either have resulted from wilful determination not to understand the representations of the New School of Zoology, as they are used by their authors and designed to be understood by them, or else the most deplorable and unpardonable ignorance of his own language, and of the figures common to all language, which a claimant for literary or scientific honours ever yet displayed. As we shall show in the sequel, the entire drift of this page, is to support the assertion that Mr. Macleay's system and "the natural system" are regarded to be identical. To contribute towards the accomplishment of this purpose, Mr. Rennie puts the word *nature* above, in italics, prefaced by a "what he calls," meaning to insinuate thereby that in the phrase "nature of the difference between affinity and analogy" is contained an implication that that difference, is, *ipso facto*, a part of "the natural system," of the system actually existing in nature. Whether this proceeds from effrontery or ignorance, it is equally astonishing; had such a line of argument been related to us of any writer, we should have denounced it as incredible, but here it is before our eyes, and we can but wonder. In order that no ambiguity or pretext for evasion may exist, we shall cite the passage of the "Dying Struggle" in which the word is used by Mr. Macleay. We shall, like Mr. Rennie, distinguish it by the *italic* character. M. Virey and Dr. Fleming having both endea-

* Linn. Trans. quoted in "Dying Struggle," p. 26.

† "Dying Struggle," p. 28.

voured to fix upon Mr. Macleay the charge of plagiarism, with respect to the distinction of relations of affinity from those of analogy, he observes, "I have however repeatedly stated that Linnæus, Pallas, and Desfontaines, and even Aristotle himself, have all mentioned certain analogies in *nature*, as distinct from affinities, before I was born. They have mentioned the existence of this distinction in particular cases; but I first pointed out its *nature* and its general application, and called the attention of naturalists to the subject." Dying Struggle, p. 25. Every reader whose progress in literature has advanced one step beyond "The London Primer," and more especially those who have ascended into the mysteries of that profound storehouse of philology and rhetoric, "Mavor's Spelling Book," will immediately perceive what Mr. Macleay means by the word *nature*, where he uses it the second time, which is that alluded to by his erudite antagonist. Far be it from us to estimate the extent of Mr. Rennie's attainments as a man of letters; but as we are so unfortunate as to differ essentially from him in opinion in our construction of this word, we must, in our own vindication, explain our views on the point. When, therefore, Mr. Macleay employs the term "*nature*" of "the distinction of relations of affinity from those of analogy," we humbly conceive that he does not intend to imply that the distinction between those relations is "nature," or the universe, as Mr. Rennie seems to think; any more than we intend, by saying that the *nature* of Mr. R's attack on the new views in Zoology is at once frivolous and unprincipled, to imply the existence of nature, or anything like nature, in any of his productions. Mr. Macleay means, that he first pointed out the *Natura* (to employ the original Latin word, in the sense in which it is used by Cicero and others) of the distinction between those two species of relation, that is, its intimate quality and peculiar characters.

This subject, as it appears to us, illustrates another on which Mr. Rennie enlarges in a previous page: "On my plan [of the study of Natural History,] any person," he observes, "may become a tolerably good naturalist, the first walk he takes in the fields, without much knowledge of books:" in illustration of this plan of study, this mode of making "tolerably good" naturalists with the rapidity of steam-power, this new royal road to the knowledge of nature, or Professor Rennie's short cut to scientific fame, we are favoured in page x, by a learned dissertation on the nest of a Dabchick, given as the great type of the proper mode of conducting ornithological investigations. But the author has unconsciously favoured us, in his construction of the word "*nature*," as above, with a memorable refutation of his own principles of study: to us this is peculiarly consolatory; for we, obtuse wights as we are, were obliged, alas, to take a great many "walks in the fields," and to pore over a great many books too, before we thought ourselves "tolerably good naturalists." But in Mr. R's mode of construing the word "nature" we now have an example before us of the discovery of a phænomenon in nidification far exceeding, in its wonderful character, what the Dabchick's nest would be, even were all the contradictory stories of it true; Mr. Rennie, *without* even a *single* "walk in the fields," has discovered by

by the mere study of books, that celebrated and interesting production of nature, often discovered before, it is true, but always of equal interest when rediscovered,—a *Mare's Nest.*—We are not fond of pleasantry in the discussions of science, but really this blunder is worthy only of ridicule.

To return however to our examination of page xxxii. In the first quotation from the Dying Struggle, Mr. Macleay certainly does strongly affirm his opinion, that the two relations of affinity and analogy co-exist in nature, (to which subject we shall by and by advert in detail,) and were designed so to co-exist, at the Creation. But all he asserts of himself, is, that he, by induction, *discovered* the nature of the distinction between them, and also the nature of their co-existence; and as truly might the Chemist who has observed that hydrogen, oxygen, and sulphur, enter into combination with each other or with other bodies invariably in proportions represented by the numbers 1, 8, and 16, or their multiples by a whole number, say, "It is quite inconceivable, that the utmost human ingenuity could make" the proportions in which these bodies combine "tally with each other, *had they not been so designed at the Creation.*" He would describe in these terms, relations which the Creator has been pleased to confer on certain forms of matter; Mr. Macleay does the same, but he does no more; neither does more or less, by using this language, than describe an ascertained phænomenon. Similar is the case with Mr. Macleay's saying that the great groups into which he has discovered the animal kingdom to be distributed, on its first ramification, "are almost mathematically proved to be natural." This is just the expression which the chemist again might use, in the actual condition of his science, respecting Dr. Prout's doctrine that all the numbers representing the proportional combining weights of the chemical elements are simple even multiples of the least of them; a doctrine which "is almost mathematically proved to be natural."

We quit the subjects of this introductory page, with the observation that the quotations in it have been made and arranged, either disingenuously or ignorantly, to prove that Mr. Macleay's system claims to be *ipso facto* the system of nature: if such be not their design, they can have been intended for no purpose whatever, nor do they serve any other. [To be continued.]

Sept. 29, 1831.

The Life of Sir Humphry Davy, *Bart. LL.D. late President of the Royal Society, Foreign Associate of the Royal Institute of France, &c. &c. By* John Ayrton Paris, *M.D. Cantab. F.R.S. &c. Fellow of the Royal College of Physicians.*

[Continued from page 223.]

In the chapter which contains the account of the decomposition of the fixed alkalies, notice is also taken of some of Davy's experiments and discoveries, which though of minor are still of great importance; to these, however, we shall but briefly allude. Among them are an investigation of the nature of Antwerp blue, which proved to be a mixture of Prussian blue and alumina; the production of the vegetation of the carbon of the wick of a candle, by

by placing it between the wires of a battery; the repetition of Gay-Lussac and Thénard's very important process for preparing potassium; and his attempts to decompose the earths. With respect to the latter Dr. Paris remarks, "his results were indistinct: they could not, like the alkalies, be rendered conductors of electricity by fusion, nor could they be acted upon in solution, in consequence of the strong affinity possessed by their bases for oxygen." While engaged on this subject he received a letter from Berzelius, announcing the fact that he, in conjunction with Pontin, had decomposed barytes and lime by negatively electrizing mercury in contact with them, and thus had obtained amalgams of the bases of those earths: these experiments were repeated by Davy with many in addition, and an account of them was read before the Royal Society on the 30th of June 1808. The memoir was entitled "Electro-chemical Researches on the Decomposition of the Earths; with Observations on the Metals obtained from them, and on the Amalgam of Ammonia."

It will not be requisite to enter into a discussion on the nature of the amalgam; the appearances which it presents have not been explained. But that hydrogen and azote are metals, or contain anything metallic, will scarcely now be maintained; the amalgamation is probably merely apparent, and the recent experiments of Mr. Daniell tend to confirm this opinion.

In 1808 Davy read his third Bakerian Lecture; the title of it is "An Account of some new Analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory." Of this excellent paper Dr. Paris gives an analysis. So sanguine was Davy's hope of decomposing some substances which have even yet resisted analysis, that he addressed a letter to Mr. Children during the progress of his experiments; in which he says, "I hope on Thursday to show you nitrogen as a complete wreck, torn to pieces in different ways."

In a letter to the above-named gentleman, dated September 23, 1809, we also meet with some opinions which subsequent investigations have by no means corroborated. "I doubt not," he says, "you have found before this, as I have done, that the substance we mistook for *sulphuretted* hydrogen is *telluretted* hydrogen, very soluble in water, combinable with alkalies and earths, and a substance affording another proof that *hydrogen is an oxide*." He says also, "I find that taking ammonium as the basis of hydrogen, according to the ideas which I stated, all the compounds will agree with the suppositions that I mentioned to you, viz. eight cubic inches of hydrogen, two of oxygen, ammonia; *four and two, water;* four and four, nitrogen; four and six, nitrous oxide; four and eight, nitrous gas; four and ten, nitric acid."

We need hardly observe that of the opinions here mentioned, that only which we have printed in italics is to be found in his "Elements of Chemical Philosophy," printed within three years from the date of this letter.

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This letter contains also an account of an experiment upon which its illustrious author most probably founded his opinion, now generally adopted, that oxymuriatic acid, as it was then called, is an elementary, or at any rate an undecomposed substance. "I have kept," he says, "charcoal white-hot by the Voltaic apparatus, in dry oxymuriatic acid gas for an hour, without effecting its decomposition. This agrees with what I have before observed with a red heat. It is as difficult to decompose as nitrogen, except when all its elements can be made to enter into new combinations." The experiments by which Davy demonstrated that oxymuriatic acid is an undecomposed body, are detailed in various papers read before the Royal Society. The changes thus effected in the views of chemists have been the subject of discussion with respect to the parties with whom they originated. "As to the claim of priority," Dr. Paris remarks, "which has been urged by several philosophers in favour of the French chemists, Davy, in speaking of Gay-Lussac's paper, published in the *Annales de Chimie* for July 1814, observes, that 'the historical notes attached to it are of a nature not to be passed over without animadversion. M. Gay-Lussac states, that he and M. Thénard were the first to advance the hypothesis that chlorine was a simple body; and he quotes M. Ampère as having entertained that opinion before me. On the subject of the originality of the idea of chlorine being a simple body, I have always vindicated the claims of Scheele; but I must assume for myself the labour of having demonstrated its properties and combinations, and of having explained the chemical phænomena it produces; and I am in possession of a letter from M. Ampère, that shows he has no claims of this kind to make *.'"

Dr. Paris has we think settled the question by reference to printed documents. "Davy published his 'Elements of Chemical Philosophy' in 1812, containing a systematic account of his new doctrines concerning the combinations of simple bodies. Chlorine is there placed in the same rank with oxygen, and finally removed from the class of acids. In 1813 M. Thénard published 'the first volume of his *Traité de Chimie Elémentaire Théorique et Pratique*,' in which he states the composition of oxymuriatic acid as follows:—'*Composition.* The oxygenated muriatic gas contains the half of its volume of oxygen gas, not including that which we may suppose in muriatic acid.' It was not until the year 1816 that, by a note in his fourth volume, he appears to have at all relaxed in his attachment to the old theory of Lavoisier and Berthollet; and it will presently appear that at the period above mentioned, iodine had been discovered, and its analogies to chlorine fully established, by the sagacity of Davy."

In his ninth chapter, Dr. Paris gives an account of Davy's "Elements of Chemical Philosophy," above alluded to, of various discoveries, and also of his work on Agricultural Chemistry. We cannot afford room for any of the Doctor's remarks on these subjects, but recommend them as well worth perusal.

* Royal Institution Journal, vol. i. p. 283.

Dr.

Dr. Paris's second volume commences with a subject of peculiar interest; we allude to the introduction of Mr. Faraday to Sir H. Davy. Dr. Paris observes that "it is said of Bergman, that he considered the greatest of his discoveries to have been the discovery of Scheele. Amongst the numerous services conferred upon science by Sir Humphry Davy, we must not pass unnoticed that kind and generous patronage which first raised Mr. Faraday from obscurity, and gave to the chemical world a philosopher capable of pursuing that brilliant path of inquiry which the genius of his master had so successfully explored."

"The circumstances which first led Mr. Faraday to the study of chemistry, and by which he became connected with the Royal Institution, were communicated to me by himself in the following letter."

"To J. A. Paris, M.D.

"My dear Sir, Royal Institution, Dec. 23, 1829.

"You asked me to give you an account of my first introduction to Sir H. Davy, which I am very happy to do, as I think the circumstances will bear testimony to his goodness of heart.

"When I was a bookseller's apprentice, I was very fond of experiment, and very averse to trade. It happened that a gentleman, a member of the Royal Institution, took me to hear some of Sir H. Davy's last lectures in Albemarle-street. I took notes, and afterwards wrote them out more fairly in a quarto volume.

"My desire to escape from trade, which I thought vicious and selfish, and to enter into the service of science, which I imagined made its pursuers amiable and liberal, induced me at last to take the bold and simple step of writing to Sir H. Davy, expressing my wishes, and a hope that, if an opportunity came in his way, he would favour my views; at the same time I sent the notes I had taken at his lectures.

"The answer, which makes all the point of my communication, I send you in the original, requesting you to take great care of it, and to let me have it back; for you may imagine how much I value it.

"You will observe that this took place at the end of the year 1812, and early in 1813 he requested to see me, and told me of the situation of assistant in the laboratory of the Royal Institution, then just vacant.

"At the same time that he thus gratified my desires as to scientific employment, he still advised me not to give up the prospects I had before me, telling me that science was a harsh mistress; and, in a pecuniary point of view, but poorly rewarding those who devoted themselves to her service. He smiled at my notion of the superior moral feelings of philosophic men, and said he would leave me to the experience of a few years to set me right on that matter.

"Finally, through his good efforts I went to the Royal Institution early in March of 1813, as assistant in the laboratory; and in October of the same year, went with him abroad as his assistant in experiments and in writing. I returned with him in April 1815,

resumed

resumed my station in the Royal Institution, and have, as you know, ever since remained there.

"I am, dear Sir, very truly yours, M. FARADAY."

The following is the note of Sir H. Davy, alluded to in Mr. Faraday's letter:

"To Mr. Faraday.

"Sir, December 24, 1812.

"I am far from displeased with the proof you have given me of your confidence, and which displays great zeal, power of memory, and attention. I am obliged to go out of town, and shall not be settled in town till the end of January: I will then see you at any time you wish.

"It would gratify me to be of any service to you. I wish it may be in my power.

"I am, Sir, your obedient humble Servant,

"H. DAVY."

The invention of the safety-lamp is a subject upon which Dr. Paris has treated at considerable length, and his account is interspersed with details of great interest.

In consequence of some dreadful explosions which had occurred in the coal-mines of the North of England, a society was formed for preventing their recurrence, by inviting the attention of scientific men to the subject, and obtaining from them any suggestions which might lead to a more secure method of lighting the mines.

"To the Rev. Dr. Gray, the present Lord Bishop of Bristol," says Dr. Paris, "who, at the period to which I allude, was the Rector of Bishop-Wearmouth, and one of the most zealous and intelligent members of the Association, I beg to offer my public acknowledgements and thanks for the several highly interesting communications and letters with which His Lordship has obliged me; and by means of which I have been enabled to present to the scientific world a complete history of those proceedings which have so happily led to a discovery, of which it is not too much to say that it is, at once, the pride of science, the triumph of humanity, and the glory of the age in which we live."

Having received a letter from Dr. Gray, as chairman of the Association, to engage him in an investigation of the subject, the following was Davy's answer.

"To the Reverend Dr. Gray.

"Sir, August 3, 1815.

"I had the honour of receiving the letter which you addressed to me in London, at this place, and I am much obliged to you for calling my attention to so important a subject.

"It will give me great satisfaction if my chemical knowledge can be of any use in an inquiry so interesting to humanity; and I beg you will assure the committee of my readiness to co-operate with them in any experiments or investigations on the subject.

"If you think my visiting the mines can be of any use, I will cheerfully do so.

"There

"There appears to me to be several modes of destroying the fire-damp without danger; but the difficulty is to ascertain when it is present, without introducing lights which may inflame it. I have thought of two species of lights which have no power of inflaming the gas which is the cause of the fire-damp, but I have not here the means of ascertaining whether they will be sufficiently luminous to enable the workmen to carry on their business. They can be easily procured, and at a cheaper rate than candles.

"I do not recollect anything of Mr. Ryan's plan: it is possible that it has been mentioned to me in general conversation, and that I have forgotten it. If it has been communicated to me in any other way, it has made no impression on my memory.

"I shall be here for ten days longer, and on my return south, will visit any place you will be kind enough to point out to me, where I may be able to acquire information on the subject of the coal gas.

"Should the Bishop of Durham be at Auckland, I shall pay my respects to His Lordship on my return.

"I have the honour to be, dear Sir, with much respect, your obedient humble Servant, H. Davy."

"At Lord Somerville's, near Melrose, N. B.

Although Sir Humphry did not immediately commence his operations after writing this letter; yet such was the vigour with which he prosecuted them, that in October he wrote the following letter to Dr. Gray:

"To the Reverend Dr. Gray.

"My dear Sir, Royal Institution, Oct. 30.

"As it was the consequence of your invitation that I endeavoured to investigate the nature of the fire-damp, I owe to you the first notice of the progress of my experiments.

"My results have been successful far beyond my expectations. I shall inclose a little sketch of my views on the subject; and I hope in a few days to be able to send a paper with the apparatus for the committee.

"I trust the *Safe lamp* will answer all the objects of the collier.

"I consider this at present as a *private* communication. I wish you to examine the lamps I have had constructed, before you give any account of my labours to the committee.

"I have never received so much pleasure from the result of any of my chemical labours; for I trust the cause of humanity will gain something by it.

"I beg of you to present my best respects to Mrs. Gray, and to remember me to your son.

"I am, my dear Sir, with many thanks for your hospitality and kindness when I was at Sunderland, your obliged Servant,

"H. Davy."

The sketch alluded to in this letter is as follows: "It possesses," Dr. Paris justly remarks, "considerable interest as an original document,

cument, displaying his earliest views, and tending to illustrate the history of their progress."

"The fire-damp I find, by chemical analysis, to be (as it has been always supposed) a hydro-carbonate. It is a chemical combination of hydrogen gas and carbon, in the proportion of 4 by weight of hydrogen gas, and $11\frac{1}{2}$ of charcoal.

"I find it will not explode, if mixed with less than six times, or more than fourteen times its volume of atmospheric air. Air, when rendered impure by the combustion of a candle, but in which the candle will still burn, will not explode the gas from the mines; and when a lamp or candle is made to burn in a close vessel having apertures only above and below, an *explosive mixture* of gas admitted *merely enlarges* the light, and then gradually extinguishes it without explosion. Again,—the gas mixed in any proportion with common air, I have discovered, *will not explode* in a *small tube*, the diameter of which is less than $\frac{1}{8}$th of an inch, or even a larger tube, if there is a mechanical force urging the gas through this tube.

"Explosive mixtures of this gas with air require much stronger heat for their explosion than mixtures of common inflammable gas*. Red-hot charcoal, made so as not to flame, if blown up by a mixture of the mine gas and common air, does not explode it, but gives light in it; and iron, to cause the explosion of mixtures of this gas with air, must be made *white*-hot.

"The discovery of these curious and unexpected properties of the gas leads to several practical methods of lighting the mines without any danger of explosion.

"The first and simplest is what I shall call the *Safe lamp*, in which a candle or a lamp burns in a safe lantern, which is air-tight in the sides, which has tubes below for admitting air, a chamber above, and a chimney for the foul air to pass through; and this is as portable as a common lantern, and not much more expensive. In this, the light never burns in its full quantity of air, and therefore is more feeble than that of the common candle.

"The second is the *Blowing lamp*. In this, the candle or lamp burns in a close lantern, having a tube below of small diameter for admitting air, which is thrown in by a small pair of bellows, and a tube above of the same diameter, furnished with the cup filled with oil. This burns brighter than the simple safe lamp, and is extinguished by explosive mixtures of the fire-damp. In this apparatus the candle may be made to burn as bright as in the air; and supposing an explosion to be made in it, it cannot reach to the external air.

"The third is the *Piston lamp*, in which the candle is made to burn in a small glass lantern furnished with a piston, so con-

* "*Olefiant* gas, when mixed with such proportions of common air as to render it explosive, is fired both by charcoal and iron heated to a dull-red heat. *Gaseous oxide of carbon*, which explodes when mixed with two parts of air, is likewise inflammable by red-hot iron and charcoal. The case is the same with *sulphuretted hydrogen*."

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structed as to admit of air being supplied and thrown into it without any communication between the burner and the external air: this apparatus is not larger than the steel-mill, but it is more expensive than the other, costing from twenty-two to twenty-four shillings.

"These lamps are all extinguished when the air becomes so polluted with fire-damp as to be explosive.

"There is a fourth lamp, by means of which any *blowers* may be examined in air in which respiration cannot be carried on: that is, the *Charcoal lamp.* This consists of a small iron cage on a stand, containing small pieces of *very well burnt* charcoal blown up to a red heat. This light will not inflame any mixtures of air with fire-damp *.

"Of these inventions, the *Safe lamp,* which is the simplest, is likewise the one which affords the most perfect security, and requires no more care or attention than the common candle, and when the air in mines becomes improper for respiration, it is extinguished, and the workmen ought immediately to leave the place till a proper quantity of atmospheric air can be supplied by ventilation.

"I have made many experiments on these lamps with the genuine fire-damp taken from a blower in the Hepburn Colliery, collected under the inspection of Mr. Dunn, and sent to me by the Rev. Mr. Hodgson. My results have been always unequivocal.

"I shall immediately send models of the different lamps to such of the mines as are exposed to danger from explosion; and it will be the highest gratification to me to have assisted by my efforts a cause so interesting to humanity."

The Safety lamp as now perfected, and the principle upon which the safety depends, are so well known as to require no further elucidation. We fully agree with Dr. Paris, that "it was the fruit of elaborate experiment and close induction; chance, or accident, which comes in for so large a share of the credit of human invention, has no claims to prefer upon this occasion; step by step may he be followed throughout the whole progress of his research, and so obviously does the discovery of each new fact spring from those that preceded it, that we never for a moment lose sight of our philosopher, but keep pace with him during the whole of his curious inquiry."

[To be continued.]

* "In addition to these four lamps, we learn from an Appendix to his paper in the Philosophical Transactions, that in the beginning of his inquiries, he constructed a close lantern, which he called the *Fire-valve lantern;* in which the candle or lamp burnt with its full quantity of air, admitted from an aperture below, till the air began to be mixed with fire-damp, when, as the fire-damp increased the flame, a thermometrical spring at the top of the lantern, made of brass and steel, riveted together, and in a curved form, expanded, moved a valve in the chimney, diminished the circulation of air, and extinguished the flame. He did not, however, pursue this invention, after he had discovered the properties of the fire-damp, on which his Safety-lamp is founded."

XLIX. *Proceedings of Learned Societies.*

BRITISH ASSOCIATION FOR THE PROMOTION OF SCIENCE, Instituted September 22, 1831.

IN our next Number we intend to give a full account of the proceedings of this Association, at the meeting lately held at York;—we shall now lay before our readers a summary of its objects and rules.

Objects.—The Association contemplates no interference with the ground occupied by other Institutions. Its objects are,—to give an additional impulse and a systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate science in different parts of the British empire with one another and with foreigners,—to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress.

Rules.—All persons who have attended the first meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its rules. The Fellows and Members of chartered Societies in any part of the British empire shall be entitled in like manner to become Members of the Association. The Office-bearers and Members of the Council or Managing Committee of all Philosophical Institutions, and other members of such Institutions recommended by the Council, or Managing Committee thereof, shall be entitled in like manner to become Members of the Association. Persons not belonging to such Institutions shall be eligible annually on the recommendation of the General Committee.

The names of persons desiring to become members shall be enrolled, and their subscriptions received by the Secretaries and Treasurer. The amount of the annual subscription shall be 1*l*., to be paid in advance upon admission, and the amount of the composition in lieu thereof, 5*l*.

The Association shall meet annually for one week or longer. The place of each meeting shall be appointed by the General Committee at the previous meeting, and the arrangements for it shall be entrusted to the officers appointed at the preceding meeting in concert with the Local Committees.

The General Committee shall sit during the time of the meeting, or longer, to transact the business of the Association. It shall consist of all Members present who have communicated any scientific paper to a Philosophical Society, which paper has been printed in its Transactions, or with its concurrence. The members of Philosophical Institutions, who may be sent as deputies from those Institutions to any Meeting of the Association, shall be members of the Committee for that Meeting.

The General Committee shall appoint annual Sub-Committees, consisting severally of the members most conversant with the several sciences, to advise together for the advancement thereof. The Sub-Committees shall report what subjects of investigation they would particularly

particularly recommend to be prosecuted during the ensuing year, and brought forward at the ensuing meeting; they shall also engage their own members, or others, to undertake such investigations, and where the object admits of being assisted by the exertions of scientific bodies, they shall state the particulars in which it may be desirable for the Committee to solicit the cooperation of such bodies. The Sub-Committees shall procure reports on the state and progress of the several sciences, to be drawn up by competent persons for the information of the Meetings.

The Committee shall appoint at each Meeting a Sub-Committee to examine the papers which have been read at it, and the register of Communications, and to report what ought to be published, and recommend the manner of publication. The author of any paper or communication shall be at liberty to reserve his right of property therein.

Local Committees shall be formed, where necessary, by the Committee, or by the Officers of the Association, to assist in promoting its objects.

The Officers of the Association,—namely, a President, two Vice-Presidents, two or more Secretaries and a Treasurer,—shall be annually appointed by the Committee.

The Accounts shall be audited annually by Auditors appointed by the Meeting.

Officers of the Association.

President, Viscount Milton, F.R.S. &c. President Elect, Rev. W. Buckland, D.D. F.R.S. &c. *Prof. of Geology and Mineralogy, Oxford.* Vice-President, Rev. W. Vernon Harcourt, F.R.S. &c. Vice-Presidents Elect, David Brewster, LL.D. F.R.S. L. & E. *Corr. Member of the Institute of France*; Rev. W. Whewell, F.R.S. &c. *Prof. of Mineralogy, Cambridge.*

Secretaries, Wm. Gray, jun. } *Secretaries of the Yorkshire*
John Phillips, F.G.S. &c. } *Philosophical Society, York.*
Ch. Daubeny, M.D. F.R.S. &c. *Professor of Chemistry, Oxford.*
Rev. B. Powell, F.R.S. &c. *Sav. Professor of Mathematics, Oxford.*
John Robison, F.R.S. &c. *Secretary of the Royal Society E., Edinburgh.*
Rev. J. Yates, F.L.S. &c. *London.*

Local Committees.

London .... G. B. Greenough, F.R.S. *Vice-Pres. of the Geol. Soc.*
R. I. Murchison, F.R.S. *President of the Geol. Society.*
Rev. James Yates, F.L.S. &c.

Edinburgh. James D. Forbes, F.R.S. &c.
J. F. W. Johnston, F.R.S.E.
John Robison, Sec. R.S.E.

Dublin .... W. R. Hamilton, F.R.S. &c. *Royal Astronomer of Ireland.*
Rev. Dr. Lloyd, F.R.S. *Provost of Trinity College.*

India.

INDIA .... George Swinton, Esq. *Chief Secretary to the Government in India,* has been requested to form a Committee at Calcutta, with the aid of Major Benson, James Calder, Esq., Dr. Christie, Sir Edward Ryan, J. A. Prinsep, Esq. and J. Herbert, Esq.

We shall give in our next Number the names of the members of the Sub-Committees, and the subjects of scientific inquiry which they have proposed. In the mean while we have the pleasure to state, that, at the request of the Association, Professor Airy has undertaken to prepare a Report on the state and progress of Astronomy; Dr. Brewster a similar Report on Optics; Professor Whewell on Mineralogy; Mr. Johnston on Chemistry; and Mr. Forbes on Meteorology.

ZOOLOGICAL SOCIETY.

July 26, 1831.—Dr. Marshall Hall in the Chair.

Specimens were exhibited of two *Mammalia,* presented to the Society by J. Boyle, Esq., Colonial Surgeon, Sierra Leone. They were the remains of animals which died on their passage homewards, and had unfortunately been put after death into brine too weak for their perfect preservation. Since their arrival at the Museum they had been transferred to strong spirit, with the view of preserving as completely as their then state would permit, specimens of so much interest. One of them was stated by Mr. Bennett to be a fully grown *Aulacodus Swinderianus,* Temm.; the other a *Lemuridous* species, which is probably the animal noticed and imperfectly represented by Bosman under the name of *Potto.* The latter was shown to be the type of a new genus, which Mr. Bennett characterized as follows:

PERODICTICUS.

Facies subproducta. Artus subæquales. Cauda mediocris. Index brevissimus, phalange ungueali solum exserto. Dentes primores supernè 4, subæquales; infernè 6, graciles, declives: canini, $\frac{1}{1}$-$\frac{1}{1}$, conici, compressi, marginibus antico posticoque acutis: molarium in maxillâ superiore primus minimus; secundus major; ambo conici; tertius acutè tuberculatus, tuberculis duobus externis alteroque interno; quartus præcedenti similis tuberculo interno majore; sequentes (in specimine juniore desunt); in maxillâ inferiore, duo conici æquales; tertius acutè externè 2-, internè 1-tuberculatus, sequentes (desunt).

PERODICTICUS GEOFFROYI. *Per. castaneus, infrà pallidior, pilis raris cinereis interjectis: vellere lanato.*

Potto, *Bosman, Guin.* ii. 35. No. 4?

Lemur Potto, *Gmel., Linn. Syst. Nat.* 42?

Nycticebus Potto, *Geoff., Ann. Mus.* xix. 165?

Galago Guineensis, *Desm. Mamm.* 104, No. 127?

Hab. in Sierra Leone.

The head is rounded, with a projecting muzzle: the nostrils are lateral, small, sinuous, with an intermediate groove extending to the upper lip: the tongue is rough with minute *papillæ,* rather large, thin and rounded at the tip, and furnished beneath with a tongue-like

like appendage, which is shorter than the tongue itself and terminates in about six rather long lanceolate processes, forming a pectinated tip; the eyes are small, round, somewhat lateral, and oblique: the ears moderate, open, slightly hairy, both within and without. The body is rather slender. The limbs are nearly equal, long, and slender: the fingers moderately long. On the fore-hands the *index* is excessively short, the first *phalanx* being concealed, and the ungueal *phalanx* (the only *phalanx* free) being barely large enough to support a rounded nail, which does not exist on the specimen, but of which there is an apparent *cicatrix;* the nails of all the other fingers are flat and rounded. Those of the hinder hands are similar, except that of the fore-finger, which, as in the *Lemurs* generally, is long, subulate, and curved. The tail is of moderate length, and covered with hairs resembling those of the body. The hairs generally are long, soft, and woolly; each of them being mouse-coloured at the base; rufous in the middle, and paler at the tip; some few are tipped with white. Hence results on the upper surface and on the outsides of the limb a chestnut colour with a slight mixture of grey: the under surface is much paler. The muzzle and chin are almost naked, having only a few scattered whitish hairs.

The measurements of the specimen are: length of the head, 2 inches 2 tenths; of the body, 6 inches; of the tail, 1 inch 6 tenths, or including the hair, 2 inches 3 tenths. The breadth of the head in front of the ears is 1 inch 4 tenths: the distance between the eyes, 4 tenths; from the anterior angle of the eye to the end of the nose, 7 tenths; from the eye to the ear, $7\frac{1}{2}$ tenths: length of ears behind, 5, of their aperture 8, breadth 5 tenths of an inch.

Anterior Limbs.	in.	*Posterior Limbs.*	in.
humerus	1·7	*femur*	1·8
ulna	2·1	*tibia*	1·9
carpus to end of 4th (longest) finger	1·8	from *os calcis* to end of 4th (longest) finger	2·3
thumb with metacarpal bone	1·0	thumb with metatarsal bone	1·1
fore-finger	·4	fore-finger (including nail 2·5)	·8
— last joint (all that is free)	·1	3rd finger	·9
3rd finger	·9	4th finger	1·2
4th finger	1·1	5th finger	·9
5th finger	·9	span	2·7
span	2·4		

By the comparative length of the tail the genus *Perodicticus* is readily distinguishable from the other *Lemuridæ*. In this, in the moderate elongation of the face, in the moderate size of the ears, in the equality of the limbs, and especially in the extreme shortness of the *index* of the anterior hands, reside its essential characters. The latter character is especially important, and may be regarded as indicating its typical station in a family, all of which are distinguished from the neighbouring groups by a variation in the form of the *index* or of its appendages. In the *Lemuridæ* generally the nail of the *index* of the hinder hands is elongated and claw-shaped, and unlike those

those of the other fingers, which are flat as in the *Monkeys*. This is frequently accompanied by an abbreviation of the *index* of the fore-hands, which becomes in *Loris*, Geoff., very considerable, and is in *Perodicticus* carried to its *maximum*, that organ being here almost obsolete.

The habits of the animal are described by Mr. Boyle as "slothful and retiring. It seldom makes its appearance but in the night time, when it feeds upon vegetables, chiefly," he believes, "the *Cassada*. It is known to the colonists as the *Bush-Dog*."

The specimen of *Aulacodus*, being fully adult, was shown to add much to the knowledge previously possessed of an animal, only one individual of which had hitherto been seen by naturalists, and that individual so young as not to have attained its perfect characters. Mr. Bennett pointed out the deviations, in the specimen exhibited, from the description published by M. Temminck in his 'Monographies de Mammalogie', and proposed the following amended generic character:

AULACODUS, Van Swind.

Dentes incisores $\frac{2}{2}$, anticè plani, scalpro cuneato, superiores profundè bisulcati: molares $\frac{4}{4}\frac{4}{4}$, lamellares: sacculi buccales 0: pedes antici digitis 4, cum rudimento pollicis; postici digitis 4: ungues, præter pollicis subplanum, falculares, fortes, supernè rotundati, infrà dilatati sulcati: cauda pilosa, mediocris, attenuata.

The deep *sulci* on the anterior surface of the incisor teeth of the upper jaw are situated nearer to the inner than to the outer edge of the tooth, and divide its face into three ridges, the inner of which is half the breadth of the middle, and the middle less than half the breadth of the outer. The molar teeth of the upper jaw have two re-entering folds of enamel on the outer, and one on the inner side; the outer passing beyond the middle of the crown, the inner central and less deeply entering: all the teeth are nearly equal in size: the anterior three are nearly square; the posterior somewhat rounded: there is no notching on the outer edge, but a distinct notch exists where the enamel folds in on the inner side, especially of the three posterior teeth. In the lower jaw the first molar has three folds of enamel on the inner side passing beyond the middle of the crown, and one small fold slightly notched on the outer: the second and third have two inner folds and one outer, all notched at the edge: the posterior is nearly similar, but more rounded behind. This system of dentition bears a greater resemblance to that of *Erethizon*, F. Cuv., than to that of any other genus of the *Rodentia*.

The covering of the *Aulacodus Swinderianus* is peculiar, consisting entirely, except on the tail, of flattened somewhat spine-like bristles, from 1 to $1\frac{1}{2}$ inch in length, the tips only of which are flexible and hair-like: the dark space which occupies the greater portion of each of the bristles exhibits a changeable metallic lustre, varying in different positions from deep steel blue to bright copper red.

The length of the body and head is 17 inches, or measured along the convexity of the back, 20: of the tail, 9: of the head, $4\frac{1}{2}$: of the fore-leg, $3\frac{1}{2}$; *tarsus* and toes, $1\frac{1}{2}$: of the *femur*, $4\frac{1}{2}$; *tibia*, $4\frac{1}{2}$; *tarsus* and

and toes, $3\frac{1}{2}$: the ear, nearly concealed by the bristly covering, is $1\frac{1}{6}$ long, and 1 inch broad.

Mr. Boyle states that this animal "is called by some the *Ground-Pig*, by others, the *Ground-Rat*. It feeds upon ground nuts, *Cassada*, and other roots. On the passage homewards it ate potatoes, and was becoming very docile."

It is very probably the "wild Rat, bigger than a Cat" mentioned by Bosman.

A small collection of *Fishes*, formed during the voyage of H. M. S. Chanticleer, and presented to the Society by the Lords Commissioners of the Admiralty, together with numerous other Zoological specimens obtained during the same voyage, was laid upon the table. It contained among others a young individual of the *Scyllium cirratum*, in the state in which it is described by Schneider as the *Squalus punctatus* : a specimen of the *Blennius pilicornis*, Cuv., described originally by Marcgrave, and remarkable for the long acicular tooth at the back of the lower jaw on each side, a peculiarity which may hereafter cause it to be regarded as the type of a distinct genus : a specimen of the *Antennarius scaber*, *Chironectes scaber*, Cuv., also described by Marcgrave : and two species which appeared to be new to science, and which were thus characterized by Mr. Bennett:

Chromis Tænia. *Chrom. brunneo-nigrescens : pinnis nigrescentibus; caudali subrotundatâ nigro fasciatim punctatissimâ: maculâ rotundâ infraoculari, alterâ ad basin pinnæ caudalis supernè, tæniâque ab oculo per medium latus ad pinnam caudalem ductâ, nigris.*

D. $\frac{15}{11}$. A. $\frac{4}{8}$. P. 13. C. 16.

Hab. apud Trinidad.

Affinis *Chrom. punctato*, Cuv., (*Labrus punctatus*, L.). Differt a figurâ Blochianâ tæniâ laterali, pinnisque haud lineatis: differt etiam numero radiorum pinnarum.

Monacanthus setifer. *Mon. caudâ hispidâ : cirris brevibus multifidis raris conspersus : pinnæ dorsalis radio 2do longissimo : pallidè brunneus, lateribus mediis nigro undulatim longitudinaliter lineatis : pinnæ caudalis rotundatæ fasciâ angustâ submediâ.*

D. 1, 28. A. 29. C. 12. P. 12.

A description, by the Rev. Robert Holdsworth, of a fish taken in the seine, at Start Bay, on the south coast of Devon, in August 1825, was read. Mr. Holdsworth regards the fish in question as the *Umbrina*, *Sciæna Aquila*, Cuv.; with which species, occasionally taken in the English Channel, his description agrees.

L. *Intelligence and Miscellaneous Articles.*

ON THE ATOMIC WEIGHT OF BARYTES. BY DR. THOMSON.

"I HAD from a set of experiments in which dry chloride of barium was decomposed by sulphate of potash, inferred that the atomic weight of barytes was 9·75. The conclusion was founded upon this experiment: 13·25 grains of chloride of barium were mixed with 11 grains

11 grains of sulphate of potash, both in solution; and it was found that after the sulphate of barytes had precipitated, the residual liquid contained no sensible quantity of sulphuric acid or of barytes. Berzelius in the third volume of his Lärbok i Kemien, stated that when these proportions were used, there always remained an excess of barytes. I requested several of my practical pupils to repeat the experiment without mentioning my object, and they all gave me the same proportions of the two salts that I had previously stated. I was induced in April 1828 to try the experiment anew, and for this purpose prepared a quantity of pure chloride of barium and of sulphate of potash. After repeating the experiment about thirty times, varied in every possible way, I found myself quite unable to determine the exact proportions of the two salts which decompose each other. There was no difficulty in finding the proportions which, when mixed together, leave no sensible residue of sulphuric acid and barytes in solution. But when I attempted to collect the sulphate of barytes and chloride of potassium, I never found the quantities to agree in any two consecutive experiments. Suspecting that the method of using double filters might be the cause of the uncertainty, I substituted single filters of Indian paper, which were finally burnt in platinum crucibles. I was therefore obliged to give up the attempt of determining the atomic weight of barytes by this method. Dr. Turner has since explained the cause of this failure, which indeed I suspected at the time. It is owing to a portion of the sulphate of potash adhering obstinately to the sulphate of barytes, and thus escaping decomposition *. Foiled in this, I substituted sulphate of ammonia, and afterwards sulphuric acid. By mixing a solution, containing a given weight of chloride of barium with an excess of sulphate of ammonia or of sulphuric acid, evaporating to dryness, and then exposing the residual salt to a strong red heat, I succeeded in determining the weight of sulphate of barytes, which is the equivalent for a given weight of chloride of barium. The same weight of chloride of barium was afterwards decomposed by nitrate of silver, and the weight of chlorine determined from my old data that chloride of silver is a compound of

Silver	13·75
Chlorine	4·5
	18·25

The result of these trials (which occupied me for several weeks) was, that sulphate of barytes is a compound of

Sulphuric acid	5
Barytes..................	9·5006

Now as 5 is the atomic weight of sulphuric acid, it is clear that 9·5 must be the atomic weight of barytes."—*System of Chemistry, (Inorganic Bodies,)* 1831. vol. i. p. 426.

Dr. Thomson observes that these experiments are confirmed by those

* Phil. Mag. and Annals, N.S., vol. viii. p. 183.

those of M. de Saussure, which do not differ so much as $\frac{1}{1000}$ from his.—*Ann. de Chim. et de Phys.* vol. xliv. p. 27.

Hydrogen being unity, Dr. Thomson's number for barytes will be 76 instead of 78 as he formerly determined; his present analysis nearly coincides with that of Berzelius. The following are the results of the three chemists mentioned.

	Dr. Thomson.	De Saussure.	Berzelius.
Acid	34·49	34·48	34·31
Base	65·51	65·52	65·59
	100·00	100·00	100·00

Edit.

ON THE OXICHLORATES. BY M. SERULLAS.

M. Serullas concludes from his experiments, 1st. That oxichloric (perchloric) acid forms with potash a very slightly soluble salt, requiring 65 times its weight of water at the temperature of 60° Fahr.

2nd. That soda forms with the same acid a very deliquescent salt, which is consequently very soluble in water, and even in the strongest alcohol.

3rd. That properties so opposite and decided, afford a method of separating potash and soda when in solution; the latter yielding, as has been already stated, an oxichlorate very soluble in concentrated alcohol, and the former an oxichlorate which is absolutely insoluble in it.

4th. That in the same experiment any acid may be separated from the potash which is combined with it; the acid being always set at liberty by the oxichloric acid.

5th. That the employment of oxichlorate of silver for the mixtures of the chlorides of sodium and potassium, and the employment of oxichlorate of barytes for the mixture of the sulphates of these two bases, renders it, by the intervention of alcohol, extremely easy to separate all the elements completely.

Oxichlorate of potash is composed of

Acid	34·275
Base	65.725
	100·000

As bitartrate of potash is soluble in 60 parts of water, and oxichlorate requires 65, oxichloric acid when added to a saturated solution of the bitartrate occasions slight precipitation.

Oxichlorate of barytes is deliquescent, very soluble in water and in alcohol; the solution when evaporated in a stove yields long prismatic crystals; paper impregnated with the solution burns with a fine green flame. It is composed of

Acid	54·423
Base	45·577
	100·000

When

When heated to redness in a tube it loses about 38·102 per cent, or seven atoms of oxygen from the acid, and one atom from the base.

Oxichlorate of Strontia.—When the solution is evaporated to the consistence of a syrup, it assumes on cooling a mass of a crystalline appearance, which readily attracts moisture from the air. It gives a fine purple to flame.

Oxichlorate of Lime.—This salt is deliquescent; when evaporated to the consistence of a syrup it solidifies into a crystalline mass. It is soluble in alcohol, and burns with a reddish flame.

Oxichlorate of Magnesia.—Deliquescent, soluble in alcohol, and crystallizes in long prisms; oxichlorate of alumina reddens litmus paper, although excess of gelatinous alumina has been used in preparing it; it does not crystallize, and is soluble in alcohol.

Oxichlorate of Lithia.—It is prepared like the preceding salts by the direct union of the acid with the base. It crystallizes perfectly in long transparent needles, which are deliquescent and soluble in alcohol.

Oxichlorate of Ammonia.—This salt is neutral; but like ammoniacal salts in general it is rendered acid by evaporation; it crystallizes in very fine transparent rectangular prisms, bevelled at the extremities. It is soluble in five times its weight of water, and slightly so in alcohol. If concentrated oxichloric acid be poured into a strong solution of this salt, a precipitate is formed which might be supposed to be a supersalt; but it is neutral, the acid having seized a portion of the water which held the salt in solution.

Oxichlorate of Zinc.—Obtained by the double decomposition of zinc and oxichlorate of barytes,—it crystallizes in prismatic groups: it is soluble in alcohol, and is deliquescent.

Oxichlorate of Manganese.—Oxichloric acid does not act upon peroxide of manganese. The oxichlorate of the protoxide is obtained by the double decomposition of oxichlorate of barytes and protosulphate of manganese. It crystallizes in long needles, is very deliquescent, and is soluble in alcohol.

Oxichlorate of Iron.—Prepared by the mutual decomposition of oxichlorate of barytes and protosulphate of iron: it crystallizes in long colourless needles, which remain long exposed to the air without alteration, but eventually they undergo a change analogous to that of the protosulphate of iron. By evaporation a portion is converted into peroxichlorate, some oxide being precipitated; it hardly melts upon red hot coals.

Oxichlorate of Copper.—Prepared by heating together peroxide of copper and oxichloric acid. By evaporation in a stove it gives bulky blue crystals which have no well determined form. This salt reddens litmus, deliquesces, and is soluble in alcohol. Paper impregnated with the aqueous solution and dried, fulminates upon burning coals with jets of fire of a very fine blue; when it burns with flame it is green.

Oxichlorate of Lead.—Prepared by heating protoxide of lead in water and oxichloric acid: it crystallizes in small prisms united into a mass; is soluble in about its own weight of water, does not deliquesce;

 its

its taste is slightly sweet and very acerb; very astringent, and much more so than acetate of lead.

Protoxichlorate of Mercury.—Dissolve fresh precipitated protoxide in the acid: by evaporation, small masses of prismatic crystals are obtained radiating from a common centre; it is not deliquescent; precipitated black by ammonia.

Peroxichlorate of Mercury.—Heat the peroxide in the acid: it reddens litmus paper whatever may have been the excess of peroxide employed. The filtered liquor strongly concentrated and put into a stove of 88° Fahr. gave very distinct colourless transparent crystals, having the form of right prisms which are so low as to be tabular;—at other times, and probably dependent upon the degree of concentration, it gave long confused prismatic crystals; but they both existed only for a short time. They dissolved in the air even in the stove. This salt is precipitated of a brick-red by potash, and white by ammonia. In alcohol it forms a white flocculent precipitate, which upon aggregating becomes reddish; it is peroxide of mercury. The solution after filtration and concentration by evaporation, is precipitated of a reddish black by potash, which indicates a mixture of protoxichlorate and peroxichlorate; when evaporated in a stove it yields, in the middle of the uncrystallizable liquor, small slender crystals, which fulminate on hot coals, and are precipitated black by ammonia.

The crystals of peroxichlorate of mercury might perhaps be preserved by puttingthe hot solution, properly concentrated, into a small bottle, and carefully corking it as soon as the crystals are formed.

Oxichlorate of Silver.—Prepared by dissolving the oxide in the acid. The solution becomes brown by exposure to the light. It did not crystallize in a stove. When dried it is a white powder, and when exposed to the air it quickly attracts moisture; concentrated alcohol dissolves it; when dry, and strongly heated in a tube, it fuses, and concretes into a mass on cooling; a small portion is transformed into chloride; it is immediately decomposed at a heat a little below redness; paper moistened with the solution, then dried at a gentle heat, detonates violently when the temperature is raised to about 400° Fahr.;—this was proved by placing parcels of the impregnated paper upon mercury heated gradually, with a thermometer placed in it.

All the oxichlorates fuse more or less vividly upon heated coals; they generally assume a prismatic form. All that have been above described are deliquescent, except the oxichlorate of lead, protoxichlorate of mercury, and the oxichlorate of ammonia. In order to obtain crystals of the deliquescent oxichlorates readily, they must be dried, dissolved in strong alcohol, and after filtration evaporated in a stove.

One of the characters which distinguishes the chlorates from the oxichlorates, is that the first, as well known, become of a deep yellow colour by the action of concentrated sulphuric or muriatic acid, while the oxichlorates submitted to the same test remain colourless.—*Ann. de Chim. et de Phys.* Mars 1831.

LUNAR

LUNAR OCCULTATIONS FOR NOVEMBER.

Occultations of Planets and fixed Stars by the Moon, in November 1831. Computed for Greenwich, by THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1831.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.	Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Nov. 8	μ^2 Sagittarii	6	2098	20 40	5 31	66	89	21 49	6 41	296	328
	16 Sagittarii	6	2099	21 5	5 57	164	190	21 28	6 19	200	229
12	45 Capricor.	6	2576	1 20	9 54	95	127	Under	horizon.	...	...
16	33 Ceti	6	125	3 8	11 27	160	184	3 56	12 15	250	279
21	E^2 Orionis..	5·6	777	4 18	12 17	62	34	5 17	13 16	310	297
23	ζ Cancri.....	6	998	0 58	8 50	58	21	1 43	9 35	298	258
24	π^2 Cancri...	6	1122	3 8	10 56	9	329	3 23	11 11	337	297
25	Regulus....	1	1209	Under	horizon	...	...	*2 54	10 37	254	217
26	Saturn......	...	...	8 55	16 33	40	15	10 3	17 41	272	258
29	ι^3 Virginis .	6	1551	Under	horizon	...	...	8 21	15 48	220	183

* At emersion, ☽ and star rising above horizon.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1831.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·302. Sept. 16. Wind N.E.—Min. 29·295. Sept. 30. Wind S.E.
Range of the mercury 1·007.
Mean barometrical pressure for the month .... 29·962
Spaces described by the rising and falling of the mercury............ 3·608
Greatest variation in 24 hours 0·316.—Number of changes 13.
Therm. Max. 71°. Sept. 5. Wind S.W.—Min. 45°. Sept. 2. Wind N.W.
Range 26°.—Mean temp. of exter. air 59°·16. For 31 days with ☉ in ♍ 60·05
Max. var. in 24 hours 19°·00.—Mean temp. of spring-water at 8 A.M. 54·01

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 27th..... 96°
Greatest dryness of the atmosphere, in the afternoon of the 3rd...... 48·0
Range of the index .... 48·0
Mean at 2 P.M. 62°·6.—Mean at 8 A.M. 70°3.— Mean at 8 P.M. 77·8
—— of three observations each day at 8, 2, and 8 o'clock .... 70·2
Evaporation for the month 2·40 inches.
Rain in the pluviameter near the ground 3·711 inches.
Prevailing wind, South-west.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 16½; an overcast sky without rain, 5½; rain, 5.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
15	7	27	0	24	23	17

Scale

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	½	1½	5	3½	8	2	6½	30

General Observations.—The weather this month has been alternately wet and dry, and the nights were generally warm.

The 1st was a cold day, with a gale from the North, and heavy rain nearly an inch in depth, which considerably lessened the temperature of the ground. Distant thunder occurred in the afternoon of the 2nd; but the night being clear, with a North-west wind, the first hoar frost appeared in the grass fields early in the morning of the 3rd. On the 7th and 8th, distant thunder occurred, and the lightning was vivid at midnight of the latter day. In the evening of the 12th a faint aurora borealis appeared from half-past 8 till 10 P.M. Early in the morning of the 19th a great number of swallows assembled, and suddenly departed for a warmer climate: they remained here this year only twenty-two weeks.

There were a few flashes of sheet lightning in the evening of the 26th. It lightened in the night of the 27th from 7 P.M. till midnight, when a change of wind to the North-east brought on a thunder-shower. Heavy showers of rain and hail in the afternoon of the 28th were succeeded by vivid lightning, from 5 till 10 P.M. Strong forked lightning, with thunder and rain, also occurred in the evening of the 30th from 6 till 9 P.M.

The mean temperature of this month is nearly a degree lower than the mean of September for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are, eleven meteors, one rainbow, one aurora borealis, lightning and thunder on five days; and two gales of wind, namely, one from the North, the other from the South-east.

REMARKS.

London.—Sept. 1. Very heavy rain. 2. Fine, with showers. 3, 4. Fine. 5. Rain in the morning: cloudy. 6. Rain: clear at night. 7. Fine: heavy rain at noon: clear. 8. Fine. 9. Rain. 10. Cloudy: fine. 11, 12. Overcast. 13—16. Foggy in the morning: fine. 17. Fine. 18. Slight haze: fine. 19, 20. Fine. 21. Rain: fine. 22. Foggy: very fine. 23, 24. Fine. 25. Cloudy: rain at night. 26. Rain. 27. Hazy and drizzly: fine. 28. Slight fog: heavy rain and thunder at night. 29. Fine. 30. Cloudy and warm: at night much thunder and lightning, with heavy rain.

Penzance.—Sept. 1. Rain. 2. Showers: clear. 3. Clear. 4. Rain. 5. Fair: rain. 6. Fair: showers. 7, 8. Showers. 9, 10. Clear. 11. Fair: rain. 12. Misty: fair. 13. Fair: misty. 14. Misty: fair. 15. Clear. 16. Fair. 17. Clear. 18. Clear: fair. 19. Showers. 20. Rain. 21. Clear: a shower. 22, 23. Clear. 24. Fair. 25. Misty rain. 26, 27. Misty. 28. Fair. 29. Fair: showers. 30. Rain.

Boston.—Sept. 1. Cloudy: rain A.M. and P.M. 2. Cloudy. 3, 4. Fine. 5. Cloudy: rain P.M. 6. Rain. 7, 8. Fine: rain P.M. 9. Cloudy: rain A.M. 10. Fine. 11. Cloudy: rain early A.M. 12—17. Fine. 18. Foggy. 19. Cloudy. 20. Fine. 21. Cloudy. 22. Fine. 23. Fine: rain at night. 24. Fine. 25. Fine: rain at night. 26. Fine. 27. Cloudy: lightning at night. 28. Cloudy: rain, with thunder and lightning P.M. 29. Rain. 30. Cloudy.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1831.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Sept. 1	29·949	29·841	29·90	29·90	29·927	29·838	29·38	58	46	57	52	60	46	59	NW.	NE.	N.	calm	...	1·50	0·640	0·950	...
2	29·934	29·848	29·92	29·90	29·969	29·883	29·34	66	44	60	50	62	45	51·5	N.	N.	NW.	NW.	...	·05	·040	...	0·40
3	29·925	29·872	29·95	29·93	29·986	29·940	29·40	68	38	65	50	63	49	54·5	NW.	NW.	NW.	NW.	0·30	...	...	...	...
4	29·855	29·732	29·75	29·72	29·876	29·844	29·25	71	61	66	55	68	61	57	SW.	SW.	SW.	calm	...	·08	·265	·085	...
5	29·923	29·900	29·82	29·82	29·938	29·898	29·23	76	58	65	51	71	59	61	S.	SW.	SW.	S.	...	·05	·630	·600	...
● 6	29·907	29·878	29·82	29·80	29·915	29·900	29·30	71	47	63	55	68	53	63	SW.	NW.	S.	E.	·20	·11	·300	·005	·32
7	29·862	29·646	29·80	29·77	29·873	29·778	29·30	68	41	61	51	64	50	56·5	SW.	W.	SW.	W.	...	·32	·245	·110	·19
8	29·707	29·637	29·75	29·75	29·759	29·700	29·12	68	47	60	49	63	49	55	SW.	W.	SW.	W.	...	·10	·575	·320	·14
9	29·745	29·582	29·80	29·75	29·766	29·646	29·07	60	51	61	49	60	49	52	NW.	NW.	NW.	calm	·15	·30	...	·060	·10
10	29·947	29·762	29·90	29·90	29·970	29·896	29·32	68	45	64	53	63	48	56	NW.	N.	NW.	NW.	...	·04	...	·025	·19
11	30·100	29·977	30·05	29·90	30·115	30·043	29·36	64	52	66	53	65	53	55·5	N.	S.	N.	NW.	...	...	·120	...	·11
12	30·228	30·196	30·10	30·10	30·239	30·206	29·57	66	49	66	58	68	56	61	N.	S.	NW.	E.	·20	...	...	·001	...
13	30·219	30·161	30·10	30·08	30·224	30·156	29·57	69	53	67	58	67	56	60	E.	SE.	E.	calm	...	...	...	...	...
☽ 14	30·141	30·129	30·00	30·00	30·173	30·135	29·52	67	43	65	58	65	49	60	E.	SE.	SE.	S.	...	...	...	...	...
15	30·170	30·145	30·12	30·10	30·214	30·191	29·55	69	47	64	54	65	52	57	W.	N.	NW.	NW.	·20	...	...	...	...
16	30·270	30·250	30·22	30·22	30·302	30·261	29·66	65	49	64	52	65	52	56	E.	NW.	N.	N.	...	...	...	...	...
17	30·242	30·192	30·22	30·20	30·274	30·204	29·64	65	47	63	49	64	54	56·5	N.	S.	NW.	NW.	...	...	...	...	...
18	30·120	30·003	30·04	29·85	30·108	30·075	29·48	70	43	63	50	66	51	58·5	SW.	SW.	S.	calm	·30	...	...	...	...
19	29·915	29·829	29·88	29·70	29·986	29·888	29·32	69	41	61	55	63	47	58	S.	NW.	S.	calm	...	·03	...	·100	...
20	29·862	29·849	29·85	29·70	29·913	29·869	29·21	66	55	61	49	62	57	55	W.	SW.	SW.	W.	...	...	·300	·340	...
○ 21	29·862	29·803	29·80	29·70	29·873	29·835	29·21	69	45	62	50	64	55	56	S.	W.	W.	W.	·25	·26	·440	...	...
22	30·018	29·932	29·90	29·90	30·075	29·888	29·32	70	37	61	48	61	47	55	SW.	W.	W.	calm	...	...	...	...	...
23	30·182	30·124	30·10	30·10	30·236	30·159	29·50	70	53	64	47	62	57	54	S.	SW.	SW.	NW.	...	...	...	...	...
24	30·230	30·147	30·12	30·10	30·272	30·209	29·47	71	51	66	56	66	54	63·5	S.	SW.	SW.	W.	·30	...	...	...	0·35
25	30·037	29·974	29·94	29·94	30·079	30·025	29·32	59	55	66	58	65	58	60	S.	SW.	SW.	W.	...	·07	·130	·030	...
26	30·023	29·970	29·94	29·94	30·045	29·975	29·41	65	49	64	52	68	58	56	NE.	SE.	S.	NW.	...	·02	...	...	·05
27	29·908	29·809	29·90	29·90	29·893	29·843	29·27	73	53	65	54	68	57	59	S.	S.	SE.	NW.	·25	·23	...	·080	...
☾ 28	29·772	29·708	29·70	29·60	29·773	29·681	29·13	75	57	63	55	69	60	59	S.	SE.	SE.	calm	...	·45	...	·600	...
29	29·615	29·602	29·50	29·34	29·611	29·589	29·02	74	52	65	58	69	58	60	S.	SE.	SE.	E.	...	...	...	·045	1·05
30	29·528	29·300	29·30	29·10	29·484	29·295	28·93	73	59	64	58	66	60	63	S.	SE.	SE.	E.	·25	·58	·340	·360	...
	30·270	29·300	30·22	29·10	30·302	29·295	29·34	76	37	67	47	71	45	57·6					2·40	4·19	4·025	3·711	2·90

Calendar of the Meetings of the Scientific Bodies of London for 1831-32.

Societies.	Time of Meeting.	November.	December.	January.	February.	March.	April.	May.	June.
Royal .... Somerset-House.	Thursday, 8½ P.M.	17, 24, 30*	8, 15, 22	12, 19, 26	2, 9, 16, 23	1,8,15,22,29	5, 12	3,10,17,24,31	7, 21
Antiquaries ... Somerset-House.	Thursday, 8 P.M.	17, 24	1, 8, 15, 22	12, 19, 26	2, 9, 16, 23	1,8,15,22,29	5, 12, 23*	3,10,17,24,31	7, 21
Linnean ...... Soho-Square.	Tuesday, 8 P.M.	1, 15	6, 20	17	7, 21	6, 20	3, 17	1, 24*	5, 19
Horticultural .. 21 Regent-Street.	Tuesday, 1 P.M.	1, 15	6, 20	3, 17	7, 21	6, 20	3, 17	1*, 15	5, 19
Society of Arts Adelphi.	Wednesd. 7½ P.M.	2,9,16,23,30	7, 14, 21	11, 18, 25	1,8,15,22,29	7, 14, 21, 28	4,11,18,25	2,9,16,23,30	6, 13
RoyalSociety of Literature St. Martin's Place.	Wednesd. 3 P.M.	2, 16	7, 21	4, 18	1, 15	7, 21	4, 18, 26*	2, 16	6, 20
Geological .... Somerset-House.	Wednesd. 8½ P.M.	2, 16, 30	14	4, 18	1,15,17*,29	14, 28	11	2, 16, 30	13
Zoological .... Bruton-Street.	Thursday, 3 P.M.	3	1	5	2	2	6, 30*	4	1
Astronomical .. Lincoln's-Inn Fds.	Friday, 8 P.M.	11	9	13	10*	9	13	11	8
Royal Institut. Albemarle-Street.	Friday, 8½ P.M.	..	..	20, 27	3, 10, 17, 24	2,9,16,23,30	6, 13	1*,4,11,18,25	1, 8
Asiatic ...... Grafton-Street.	Saturday, 2 P.M.	..	3, 17	7, 21	4, 18	3, 17	7	5, 19	7*, 16 July, 7, 21
Geographical .. 21 Regent Street.	Monday, 9 P.M.	14, 28	12	9, 23	13, 27	12, 26	9, 23	14*, 28	11, 25

* ANNIVERSARIES.—Royal Society, Nov. 30, 11 A.M.—Astronomical, Feb. 10, 3 P.M.—Geological, Feb. 17, 1 P.M.—Antiquaries, April 23, 2 P.M—R. Soc. of Literature, Apr. 26.—Zoological, Apr.30, 1P.M.—R. Institution, May 1.—Horticultural, May 1, 1 P.M.—Linnean, May24, 1 P.M.—Asiatic, June 7, 1 P.M.—Geographical, May14, 1P.M.

COMMITTEE OF SCIENCE AND CORRESPONDENCE OF THE ZOOLOGICAL SOCIETY meet on the second and fourth Tuesdays of each Month, at 9 P.M.

[Copies of the Calendar on Cards may be had at the Office of the Philosophical Magazine and Annals, Red Lion Court, Fleet Street.]

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

DECEMBER 1831.

LI. *On Isomorphism. In Reply to* Mr. Brooke. *By Professor* Whewell, *of Cambridge* *.

IN the Phil. Mag. and Annals for September last, appeared some observations "on Isomorphism," by Mr. Brooke, in which a very unfavourable opinion was expressed of the doctrine so designated, and of its probable effect on the progress of mineralogical chemistry. Everything on this subject, which comes from a gentleman of such exact acquaintance with minerals as Mr. Brooke possesses, is well deserving notice; and the high claims which have been set up in favour of the theoretical and practical consequences of the doctrine in question make it very desirable to ascertain distinctly how far its pretensions are well founded. I am one of those who look to it for the solution of many difficulties in mineralogy, hitherto insurmountable, and who expect that it will lead us to the true line of junction between chemistry and crystallography. I shall be glad, therefore, to state my views of the evidence on which the doctrine of isomorphism rests, and of the validity of its proof as compared with that of the theory which Mr. Brooke opposes to it. I will begin with this last.

I. *On the Theory of essential Ingredients and accidental Mixture.*

The old theory of the constitution of minerals, to which Mr. Brooke seems inclined to adhere in opposition to the doctrine of isomorphism, is, that each mineralogical species consists of certain parts which are essential; and that the varieties of composition which we find in actual specimens arise from the mixture of extraneous substances with these fundamental elements. Thus Mr. Brooke supposes, at least

* Communicated by the Author.

as an illustration, " that amphibole consists essentially of a single atom of trisilicate of lime, and that all else which may be discovered by analysis is accidental mixture."

The difficulties of this theory appear to me to be absolutely unconquerable; and if such were the best conjecture we could make concerning the constitution of our mineral specimens, we should be compelled, I think, to despair of ever attaining any distinct or consistent knowledge of them. For if we take any table of the analyses of amphibole, for instance that in Leonhard's *Handbuch*, we find, besides the lime and silica, which Mr. Brooke supposes to be essential, a proportion of magnesia varying from 2 to 25 per cent., protoxide of iron from 0 to 20 per cent., alumina from 0 to 26 per cent. and various other ingredients. The smallness of the amount of these ingredients in some cases, shows that, on any hypothesis which looks to essential elements, they are not essential; while the largeness of their amount in other cases shows that we can acquire no useful or applicable knowledge of the composition of minerals by taking up a view in which they go for nothing.

If we are to neglect 25 per cent. of magnesia or of alumina in some specimens of amphibole as accidental mixture, by what right do we take account of the lime as essential, which in no case amounts to more than $14\frac{1}{4}$ per cent.? The chemical constitution of a mineral according to such a view must be a matter of mere assertion; for it not only does not approximately represent *all* the good analyses (which is what it ought to do), but it does not represent *any one*, within any reasonable limits. It does not represent the analyses any better than a dozen other suppositions would do taken at random. It appears to be, on this theory, a matter of arbitrary assumption which part of the analysis is to be supposed essential and which accidental: and if we could not find any better mode of considering our analyses, I cannot conceive what reason there could be for ever analysing a mineral at all. In interpreting the result, we should always have the difficulty which Dr. Johnson complained of with regard to an inaccurate narrator;—it is of no use to be content with believing half of what he says, for we do not know *which half*.

In some cases we may go further, and show that there *cannot* be any essential composition, common to all the specimens of the same species. What are the essential ingredients of garnet? If we again turn to Leonhard's table, we find silica indeed common to all the analyses, but not one single ingredient besides. Alumina, lime, protoxide of iron, peroxide of iron, protoxide of manganese, are the most common of

of the other ingredients; but there is not one of these which is not, in more than one case, either absent, or present in very small quantity. Shall we say then that garnet consists essentially of silica, and that the other substances are accidental? that it is thus chemically identical with quartz? This appears too absurd to be thought of; and yet the doctrine of essential composition with accidental mixture appears to leave us no other alternative*.

A case often quoted by the advocates of accidental mixture is the Fontainbleau spar, and this Mr. Brooke adduces. This instance has always appeared to me utterly irrelevant. The carbonate of lime is here crystallized among the previously existing particles of sand; so that these are imbedded in it like plums in a pudding. There is no difficulty in such a case, any more than when any one mineral is crystallized about splinters or needles of another;—a very common occurrence. But who would consider these splinters or needles as belonging to the surrounding crystals? In the Fontainbleau spar, no one doubts that the carbonate of lime gives the crystalline character, and that the quartz is caught up in it in grains. The slightest touch of acid, or scratch with the penknife, proves this. It is the easiest thing in the world to show that this is *not* a homogeneous or simple mineral: the question is about such as *are* homogeneous.

Mr. Brooke would perhaps reply to this, that the Fontainbleau spar is a palpable mixture, and that other minerals may be impalpable mixtures: that they may appear to be homogeneous when they are not really so: and that there may be a gradual transition from mixtures obvious to the eye, like the Fontainbleau spar, to mixtures which cannot be detected by any physical character, and are discoverable only by chemical analysis.

To this I reply, that if we really cannot tell whether a specimen be approximately a simple mineral, I do not see what the use can be of analysing it at all. But the way to decide this point, is to take minerals which possess the obvious qualities which may be expected to characterize simple minerals; as transparency, smooth surfaces, apparently uniform structure, bright and uninterrupted cleavage extending to the smallest fragments: all which properties the Fontainbleau spar wants. And if we do take *such cases*, we neither find ourselves driven to specimens manifestly very impure; nor do we find by chemical examination, large and irregular accidental mixtures. Who ever detected, in apparently pure quartz, 10 or even 5 per cent. of extraneous mixture? Who ever discovered in pellucid

* See our present Number, p. 424.—Edit.

 calc-

calc-spar any considerable per centage of silica? If we wish to know the chemical constitution of minerals, we must take those which are apparently simple, pure, and of determinate character. If we take those which are plainly impure, mixed, and indefinite, we are not to be surprised if we find the chemical composition also perplexed, variable, and anomalous. What can we learn by looking, in the first instance, to cases which we know to be exceptions to all rule, and which no theory pretends, or can be expected, to include?

The fact which has hitherto so perplexed mineralogists is, not that *impure* specimens are of anomalous composition; but that specimens, apparently pure, homogeneous, and well crystallized, vary in their ingredients in an unaccountable manner: and it is this variation which the doctrine of isomorphism undertakes to reduce to laws; so that the analyses of all well selected specimens of the same species shall, within reasonable narrow limits, agree with the theoretical constitution.

If this theory do not teach us the nature of mineral species, it at least promises fairer than that of accidental mixture. If we learn little by comparing our analyses with a standard with which we conceive them to agree (the isomorphous composition of the species) we shall learn still less by comparing them with an assumed standard (the essential ingredients) from which it is allowed that they may differ widely and by an unknown quantity. If the half of our specimen may be something different from what it seems, it does not appear what hope we can have of connecting what *seems* with what *is* in minerals. If extraneous substances to the amount of 50, 60, or 70 per cent. may be "cemented together and cased up" in the pure mineral, without our senses detecting the want of simplicity, how can we ever distinguish between the case and its contents?

I shall suppose then that when we analyse a well-crystallized, well-characterized mineral, the analysis gives us an approximation to a chemical constitution belonging to the species. If this be not so, and if we do not get an approximation this way, it appears pretty clear that we shall not obtain such information in any other manner. Any theory which, like that of accidental mixture, supposes that this *may be* no approximation at all, not only disables us from inferring anything from one analysis, but makes any possible conclusion from any number of analyses, entirely arbitrary, conjectural, and precarious in the first place; and in the next, utterly inapplicable to any new instance: while in some species, as we have mentioned with regard to garnet, we can absolutely prove a negative against that theory, and show that no one possible chemical constitution can answer to the various results of analysis, even allowing admixture to any extent.

II. *On*

II. *On the Evidence of the Doctrine of Isomorphism.*

It has already been said that analyses of the same mineral often give results so widely different that it is hopeless to attempt to discover in them the same chemical compound of the same ingredients. This being so, we find that a law nevertheless does prevail in the analyses of the same species, connecting them together in a definite and certain manner, and bringing them under a common formula. This law is given by the doctrine of isomorphism. The evidence of the doctrine must depend upon the accuracy with which it represents the facts; and that the reader may judge of this, I will take the analyses of eight varieties of garnet, given by Leonhard in his account of that species (*Handbuch*, p. 489). Nothing can apparently at first sight be more anomalous than these analyses. The alumina varies from 0 to 22 per cent., the lime from 0 to 34, the magnesia from 0 to 13, the protoxide of iron from 0 to 34, the protoxide of manganese from 0 to 7. If we can find any law which approximately includes these eight analyses, we shall have strong reason to believe that it has some foundation in nature.

The following is the constitution of each of the eight specimens, expressed in atoms of the ingredients, and reduced to such a scale that the atoms of silica are 4.*

Ingredients.	S	A	Fes	Fe	Mn	C	M	K
One Atom.	59·6	64·2	97·8	87·8	91·2	71·2	51·7	
(1)	4	1·82	...	2·25	·43			
(2)	4	1·67	...	2·14	·32	·09		
(3)	4	1·86	...	1·63	·15	·40	·47	
(4)	4	1·25	·90	...	·09	2·65		
(5)	4	...	2·02	...	·53	2·58	...	·98
(6)	4	...	2·11	...	·22	2·74	...	p. cent.
(7)	4	1·84	·30	...	·03	2·88	...	2·35
(8)	4	1·97	...	·59	·39	·51	1·43	p. cent.

If

* I have in this paper used the notation which I have elsewhere recommended (Journal of the Royal Institution, No. 111). In this, S represents silica, A alumina, C lime, M magnesia, K potassa, Fe and Fes the protoxide and peroxide of iron, in which the proportions of oxygen are as 2 : 3; Mn the protoxide of manganese; as, arsenic, s sulphur, c′ carbonic acid. I have used the atomic numbers of Berzelius.

In the Phil. Mag. and Annals for August, Mr. Prideaux has dissented from some of the views concerning chemical notation to which I have just referred. I will

If now in the analyses thus expressed, we add together the numbers in the columns A and Fes, and also those in the columns Fe, Mn, C, M, we have certain numbers which we must compare with each other, as follows:

	S	A, Fes	Fe, Mn, C, M.
(1)	4	1·82	2·68
(2)	4	1·67	2·55
(3)	4	1·86	2·65
(4)	4	2·15	2·74
(5)	4	2·02	3·17
(6)	4	2·11	2·96
(7)	4	2·14	2·91
(8)	4	1·97	2·92

Now in these columns the approximate equality of the numbers in each column is remarkable. None of those in the second column differ much from 2; none of those in the third column much from 3; the first column being invariably 4. We have a degree of resemblance altogether different from what appeared in the former table. And this becomes stronger when we notice, that in all the cases the number in the third column is very nearly one and a half times that in the second, and that this proportion obtains even where the numbers themselves differ from the average, as in (2). We may observe that in this analysis (2), which is much the furthest from the mean, we have only to suppose an excess of 7 per cent. of silica, in order to make it agree with the rest; and that in this case there *must* be *some* inaccuracy in the analysis, since the proportions of the separate ingredients amount to 102 instead of 100. In (4), where the second column is too large, and the third too small, we are led to ask, whether it is possible that in the analysis any portion of Fe has been converted into Fes: the probability of such an inaccuracy must be left to chemists to determine.

But even without making any allowances, I think the agreement of the above numbers is as close as we could expect,

I will leave the matter to be decided by those who may attend to it. I shall use the notation which I have explained, because it is the only one which will answer my purpose, as well as the only one which is algebraically consistent. Those who do not find any inconvenience, or see any incongruity, in that of Berzelius, have no reason for relinquishing it. The relative advantages of the two will be discovered by any one who has to work with them: and there would be little use in a further discussion of these on general principles.

and

and sufficient to justify us in considering 2 and 3 as the standard values of the second and third columns, from which values the deviations are comparatively inconsiderable.

It appears then that all the above discordant analyses are nearly represented, and most of them *very* nearly, by this law:—that the atoms of the ingredient S, those of A and Fes taken together, and those of Fe, Mn, C and M, taken together, are respectively as the numbers 4, 2, 3. If we put this law into a formula, according to the notation explained elsewhere, it will stand thus:

$$\text{Garnet} = 4\,S + 2\,A, Fes + 3\,Fe, Mn, C, M;$$

which may also be put in this form,

$$2(S + A, Fes) + 2S + 3Fe, Mn, C, M.$$

Or this, $2(S + A) + 2S + 3C$:

where, in the latter formula, it is understood that a portion of A may be replaced by Fes, and a portion of C by M, Mn, or Fe.

Since we can include the above analyses under a simple arithmetical law by supposing such replacements, while we can exhibit no such law without this supposition, this example appears to prove the usefulness of the doctrine that ingredients do thus replace each other; and this is the doctrine of isomorphism. Such is the kind of evidence on which the establishment of this doctrine must rest; and its certainty will depend on the exactness with which it will thus reduce to a common formula a number of analyses of minerals, agreeing in crystallographical and physical character. The above case is taken unfavourably for the doctrine of isomorphism; for if several different chemical formulæ ever meet under one crystalline form, this is perhaps most likely to occur in crystals of the tessular system, like garnet. It has however, I think, been shown, that in the case thus taken, the formula includes all the analyses, at least within 4 or 5 per cent.; while the doctrine of essential constitution is absolutely inapplicable, inasmuch as silica is the only ingredient common to all the cases.

A similar examination of a number of analyses of amphibole, with a similar result, is given by Bonsdorf in the *Ann. de Chim.* tom. xx., and a similar discussion of pyroxene, by Rose, in the *Ann. de Chim.* tom. xxi. Undoubtedly in various other minerals the true constitution remains still to be discovered, and can be obtained only by the laborious process of making or collecting many good analyses of the mineral, well ascertained and pure, and then comparing these in a manner somewhat similar to that employed above, so as to discover

discover the proper formula. This labour has, as yet, been executed for a few minerals only; so that there remains still abundant occupation of this kind for the mineralogist, and it can hardly be doubted, that a clearer insight into the constitution of crystalline substances will reward such researches, if diligently pursued. But the above instances, in which, in minerals of the most complex and apparently confused analysis, order has been detected by the application of the doctrine of isomorphism, appear to be sufficient to assure us that that doctrine has a foundation in nature.

If there be any instances in which the analyses do not conform to the formula proposed, as Mr. Brooke asserts to be the case with regard to amphibole compared with Beudant's formulæ, there can be no doubt that we must allow either that the formula is wrong, or the mineral wrongly named, wrongly analysed, or impure. The isomorphous view of the constitution of bodies has not, nor can it have, any authority beyond what it derives from its agreement with facts. The superiority of this view over that of accidental mixture, arises from its appearing that the latter gives no approximation at all to many of the analyses; while the former has given, in the cases which have been most carefully examined, a very close agreement, under very trying circumstances.

III. *On the Principles of the Doctrine of Isomorphism.*

Besides Mr. Brooke's objections to the doctrine of isomorphism as not supported by sufficient evidence, he has put forward several objections founded upon theoretical views, which I shall very briefly touch upon.

In p. 164, he says: "If soda and lime are isomorphous in relation to 1 or 2 or 3 atoms of silex, there is no obvious reason why all the other elements that are deemed isomorphous in relation to 1 atom should not be so equally in relation to 2 or 3 atoms," &c. Mr. Brooke here and in other cases appears to assume, that if any two elements x and y be isomorphous in one case, they must be so in all: and this because there is no *obvious* reason why it should be otherwise. I confess this appears to me a hazardous mode of reasoning in such matters. Whether the fact is so or not, must be determined by analyses, and perhaps is hardly yet decided. The elements which I have denoted by C, M, Fe, Mn, which appear to be *isomorphous* in siliceous minerals, are, it would seem, *plesiomorphous* when combined with carbonic acid.

We must, I conceive, hold such ingredients in each mineral species to be isomorphous, as appear, from analyses, to replace each

each other *in that species*. The general laws of isomorphous elements are as yet imperfectly understood; and when we have but one analysis, it must often be difficult or impossible to decide whether any two ingredients are in definite proportions, or in isomorphous or plesiomorphous relation.

There are many, therefore, of the general arguments employed by Mr. Brooke which involve reasonings unauthorized by anything yet established as part of the doctrine of isomorphism.

IV. *On Plesiomorphous Groups.*

We find in many cases minerals which, agreeing in the form of their chemical formula, and differing in one or more of their elements, agree also in their system of crystallization, but differ slightly in their angles and in their physical properties. Thus carbonate of lime, of iron, of magnesia, ($C + 2c'$, $Fe + 2c'$, $M + 2c'$,) and mixtures of these, all belong to the rhombohedral system, and have a certain approximate agreement of hardness, scratch, lustre, cleavage, &c. which gives them a general resemblance; while their angles vary from 105° 5′ to 107° 40′, according to the ingredients. Such minerals have been termed *plesiomorphous;* and it is evident, as Mr. Brooke allows, that if we can range minerals into such groups, we shall have made an important step in mineralogy. "If," he says, "the class of primary forms can be indicated with certainty by the chemical composition of a crystallized body, a benefit will so far have been conferred on science." Several such groups appear to have been already detected: thus, besides the rhombohedral group of the form $R + 2c'$ already noticed, we have the carbonates of baryta, strontia, lead, and lime (arragonite), which are prismatic; the sulphates of the same bases, which are also prismatic; and we have a similar group in the various species into which mineralogists have subdivided the siliceous minerals formerly included under the name *felspar*. Mr. Miller has suggested the possibility of another group, containing the silicates of zinc, of iron (yenite), of magnesia and iron (peridot) (Camb. Trans. vol. iii. p. 419). A circumstance which gives additional importance to these groups is, that besides the agreement in the system of crystallization, and the close approximation of their angles, they are always found to possess several other physical properties in common, or with slight differences.

M. Beudant has asserted a proposition concerning the dependance of the angle on the chemical constitution in these groups, which is probably not true; as Mr. Brooke has very satisfactorily shown. He has maintained, that the angle of

 $C + 2c'$

$C + 2c'$ being 105° 5′, and that of $M + 2c'$ being 107° 25′, the angle of any mineral of the kind $C, M + 2c'$, will be intermediate between the two angles, exactly in proportion to the mixture of C and M in its composition. Mr. Brooke has pointed out that this doctrine is not reconcilable with the measured angle and ascertained composition of Breünnerite. Indeed it does not appear that M. Beudant has attempted to establish his law by a series of measures and analyses, and it certainly cannot be established in any other way. It would be highly interesting and important to mineralogy to determine the rule according to which the alteration of the ingredients affects the angle of the crystal; but it is not likely that this will be done by assuming the first arbitrary conjecture as the true law of nature.

It is possible that some of the groups held to be isomorphous may be plesiomorphous only; and that more exact measurements may detect differences in angles hitherto supposed to be equal. This discovery would by no means diminish the importance of the study of those substitutions of ingredients by which resembling minerals are related; and this study is undoubtedly one on which the progress of mineralogy will much depend.

V. *On Dimorphous Substances.*

It is well known that certain substances exhibit the curious phænomenon of two different fundamental crystalline forms, without our being able to detect any difference in the chemical constitution of the bodies. Thus $C + 2c'$ appears as a rhombohedron in calc-spar, and as a prism in arragonite. Sulphur is either a right or an oblique prism *. Such *dimorphous* substances certainly offer a difficulty in any attempt to connect the crystalline form with the chemical constitution; but they present no peculiar difficulty to the theory of isomorphous or plesiomorphous substitution. The theory of essential ingredients is at least as much embarrassed by these cases. If all that is essential to rhombohedral calc-spar be a certain proportion of lime and carbonic acid;—if this essential composition can impress the character of such calc-spar on 50 per cent. of silica;—how is it that sometimes, when there is not 1 or ½ per cent. of foreign admixture, we have quite a different crystalline form, as in arragonite? Certainly the theory of accidental mixture has here no superiority; while that of isomorphism does on the other hand fall in exactly with the fact,

* From Mr. Brooke's observations (Ann. Phil. Dec. 1823) it appears to follow that sulphate of nickel is dimorphous, crystallizing both in right rhombic prisms and in square prisms.

that

that in some specimens of arragonite a portion of strontia enters into the composition of the mineral; which is what we might expect, carbonate of strontia having a plesiomorphous relation to arragonite.

In other cases, such as those adduced by Mr. Brooke, of paranthine and sodalite; of eudialyte, zircon, and olivine; it is very far from being clearly established, as yet, that the chemical formulæ of these groups can be made respectively identical, even by the substitution of isomorphous elements; and it certainly has not yet been proved that *in these cases* the elements are isomorphous. If there be really dimorphous minerals of this kind, the observations of the last paragraph may be applied to them: at present we do not appear to possess a sufficient number of good analyses of these species, to be able to determine how far they are examples of the theory. But certainly the theory of accidental mixture cannot possibly explain any difficulties of this kind, which the theory of isomorphism will not explain at least as well.

I am therefore surprised that Mr. Brooke should have brought forwards such cases as objections to the isomorphous theory. But I am still more surprised that he should have brought forwards as objections the differences which occur in other cases, where the very essence of the theory requires that there should be a difference, and when the theory could not stand a moment if there were none. "Arsenic," he says, "combines with sulphur in two different proportions, and producing different primary forms. Hence the proportion of a common element, and therefore an isomorphous one, occasions a change, even in the system of crystallization." No doubt the change of the *proportion* of an element occasions a change in the crystallization: we should have little chance of finding any connection between the elements and the crystallization if it were not so: as + 2s (realgar) and as + 3s (orpiment) have different chemical formulæ, and accordingly we find they have a different system of crystallization. How Mr. Brooke conceives that the theory of isomorphism can identify these two compounds, I cannot understand. If, when he says, "a common element, and *therefore* an isomorphous one," he means that *two* atoms of an element are isomorphous with *three*, he certainly supposes what no advocate of isomorphism ever dreamt of.

In like manner I cannot see how the isomorphous theory offers any ground for the expectation, of which he speaks, that the sulphuret of silver, the sulphuret of copper, and the sulphuret of bismuth should present similar forms. For it is not asserted, so far as I am aware, either that the number of

atoms of sulphur is the same in each of these minerals, or that these metals can *ever* replace each other as isomorphous ingredients; so that all ground of comparison vanishes.

I will conclude with noticing the obligation which the doctrine of isomorphous and plesiomorphous groups, as well as other parts of mineralogy, owes to Mr. Brooke himself. Thus, among other instances, (Ann. Phil. Aug. 1823,) he has remarked the near agreement in form, of sulphate of iron and sulphate of cobalt: also (Ann. Phil. Dec. 1823) that of the sulphate of nickel and the sulphate of zinc; and (Ann. Phil. Jan. 1824) the exact agreement of nitrate of lead and nitrate of baryta.

Trinity College, Cambridge, Nov. 7, 1831. W. W.

LII. *Unequal Refrangibility of Light on the undulatory Theory. By* A CORRESPONDENT.

THE undulatory theory of light has now been shown to apply with such distinguished success to every phænomenon, even the most recondite and complicated which the researches of physical optics have disclosed, that no other doubt can remain on the mind of the competent examiner of its doctrines than that which results from the single exception to its universal application; viz. the explanation of the phænomenon of the unequal refrangibility of the different rays of light.

In regard to this apparent exception several suggestions have been made possessing high claims to attention. Without discussing their merit, it may be permitted to the writer of these lines to propose another which seems to him more free from objections than many of those which have preceded it.

It will be admitted that the same difficulty which attaches to the explanation of the modifications which the undulations undergo within the refracting medium, applies to the conception of their condition in the medium out of which they enter it. If we have only one homogeneous medium, it seems impossible to conceive more than one kind of undulation going on at the same time in it; the elasticity being an essentially constant element. If we could by any possibility conceive different elasticities coexisting, and by consequence, vibrations impressed with different velocities giving rise to undulations of different lengths at the same time, these would of course be unequally retarded on entering the denser medium, and unequal refraction would take place.

The suggestion now offered consists in this,—that such a coexistence

coexistence of unequal elasticities is not only possible, but must be the case; for if the æther be an elastic fluid uniformly diffused through space absolutely devoid of other matter, it follows that wherever it penetrates a space filled with any medium, such as air, it must necessarily be attracted round each particle of air, and form *spheres of a density increasing towards their centres.* Amongst an infinite number of such spheres uniformly diffused, a succession of vibrations communicated in a given direction, will of course give rise to vibrations propagated with various velocities, according to the particular elasticity of different parts of the disseminated medium: thus we shall have a number of coexistent vibrations producing undulations of different lengths which, when they are incident upon a new medium, will cause a deviation in position proportional to the unequal lengths of their undulations, according to the well known and established explanation of the general law of simple refraction as expressed by the undulatory theory.

November 1st, 1831.

LIII. *On the Symmetrical Functions of a specified Number of the Roots of an Equation. By* the Rev. R. Murphy, *Fellow of Caius Coll. and of the Camb. Phil. Soc.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

In a paper published in the last Number of the Transactions of the Cambridge Philosophical Society, I have given a few simple rules relative to the solution of algebraical equations, with their demonstrations. The sum of any *specified number* of the roots taken in order from the least, upwards, and the sum of any given function of such roots, may be thence found, for any proposed equation $\varphi(x) = 0$, containing only positive and integer powers of x. The coefficients of the different terms of an equation are, as is well known, the sums of symmetrical functions of *all* the roots; and my present object is to show a simple method of obtaining the corresponding sums of the symmetrical functions of a *specified number* of the roots; and as general investigations relative to a specified number of the roots are I believe new in analysis, this paper may not be unacceptable to your mathematical readers.

Let $\alpha_1, \alpha_2, \alpha_3 \ldots\ldots \alpha_m$ be the m least roots, taken in order, of the equation $\phi(x) = 0$, and λ any arbitrary quantity. Then by a rule given in (§ 3) of the paper above referred to, we get log

$\log(1+\lambda\alpha_1) + \log(1+\lambda\alpha_2) + \ldots\ldots \log(1+\lambda\alpha_m) =$ coefficient of $\frac{1}{x}$ in $\frac{-\lambda}{1+\lambda x} . \log\frac{\phi(x)}{x^m}$. This coefficient is a function of λ, suppose $\psi(\lambda)$. Hence $(1+\lambda\alpha_1) . (1-\lambda\alpha_2) \ldots\ldots\ldots (1+\lambda\alpha_m) = \varepsilon^{\psi(\lambda)}$ ε being the base of Napier's logarithms.

Let $\varepsilon^{\psi(\lambda)}$ be expressed in powers of λ, the general term being $u_p . \lambda^p$, and suppose we equate the terms involving like powers of λ, on both sides of the equation; then

First; When $p < m$, u_p is evidently the sum of the products of the m least roots, taken p and p together.

Secondly; When $p = m$, $u_p =$ the product of the m least roots.

Thirdly; When $p > m$, u_p is zero,

From the third case, it follows, that in applying this method we may always reject from $\psi(\lambda)$ all the terms after that which involves λ^m.

A process exactly similar will give the combinations p and p together, of any *functions* of the m least roots or their continued product. I am, Gentlemen, your obedient Servant,

R. MURPHY.

LIV. Decas tridecima Novarum Plantarum Succulentarum; *Autore* A. H. HAWORTH, *Soc. Linn. Lond.—Soc. Horticult. Lond.—Soc. Cæs. Nat. Curios. Mosc.—necnon Soc. Reg. Horticult. Belgic. Socius: &c. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

AFTER a longer space than usual between my communications to your valuable Magazine and Annals, I send you hereunder my thirteenth Decade of New Succulent Plants, for insertion, if you please, in an early Number of that useful work.

As heretofore, I have carefully stated the native countries of every species,—detailed, but with due precision, their botanical characters; and not failed to record the rich gardens from whence I have received them. And to all these particulars I have added such contrasting and other characters as may enable the gardener to grow them, the botanical tyro to understand them, and every able author to appretiate and blend them with their old affinities in a scientific way.

I remain, Gentlemen, yours, &c.

Chelsea, Aug. 30th, 1831. A. H. HAWORTH.

Ord.

Ord. Nat. BROMELIACEÆ, *Juss. Gen. Pl.* 49.
Genus Agave, *Linn. Gen. Pl.* 582.

univittata. A. (red-edged white-striped): foliis latè lorato-
1. lanceolatis acuminatis pungentibus, margine rufo aculeato-serrato, costa univittata.

Habitat in Mexico. G. H. ♃.

Ex regio horto Berolinense communicavit amicus Dom. F. Otto, A.D. 1830.

Obs. Proxima fortassè *Ag. angustifoliæ* Nob. at *folia* magìs patula seu inflexo-concava, erecto-expansa vel parùm recurvantia crassa dura seu loreo-rigida nitentia, atro-viridia, *costâ* latè albâ, in spinâ atro-rufâ validâ affinium desinente; *aculeis* marginalibus distantibus validis adunco-respicientibus atro-rufis, *margine* ipso tenuiter cartilagineo etiam atro-rufo; mox albicante periente: *subtùs* (folia) convexa pallida sive parùm albicantia (lateribus altè viridibus) lineolis numerosis parallelis contiguis longis sed deorsum sæpiùs interruptis atro-viridibus basin versus plùs minùs evanescentibus.

Juvenem plantam solùm habeo, foliis adhuc vix pedalibus 9–10 lineas latis.

Fortassè varietas vittata et lineolata præsingularis plantæ originalis quædam *absque* vittâ vel lineolis; nihilominùs propriæ species.

Ord. Nat. TULIPACEÆ, *Kunth Synops.* 1. 292.

Genus Yucca *Auctorum*, et *Nob. in Suppl. Pl. Succ. p.* 31. charactere novo ampliori.

Sectio * * * integerrimæ: foliis margine integerrimo lævi, caudice 1–10-pedali valido recto. *Nob. l. c.*

aletriformis. Y. (The undulate-leaved): foliis capitatis erecto-
2. recurvis undulatis lorato-attenuatis longè acuminatis viridibus nitidis lævibus, margine ipso cartilagineo albicante integerrimo.

Habitat Capite Bonæ Spei, et exindè ad regium hortum Kewensem misit, A.D. 1823, amicus peregrinator Dom. Bowie; adhuc non floruit in horto, sed ibi pulchrè viget, *Aletrem* speciem simulans. G. H. ♄.

Obs. Caudex adhuc bipedalis firmus teres diametro sesquiunciali.

Folia eleganter capitatim collecta (capite longiore quàm in plurimis) et concinnè basi imbricata: nunc in nostro exemplo subbipedalia, 15–16 lineas lata, sublorea, basin versus crassiora et succulentiora, supra medium

medium vix undulata sed pedetentim prodeuntia in acumine longo; *suprà*, præcipuè basin versus, inflexo-canaliculata, *subtùs* obtusè carinata, *carinâ* supra medium sensim sensimque evanescente.

Ord. Nat. CRASSULACEÆ, *DeCand. Prod. v.* 3. *p.* 381.

Tribus 1. CRASSULEÆ, *DeCand. Prod. ib.*

Genus SEDUM *Linn. &c.*

Sectio ACUTIFOLIA *Nob.*

subclavatum. S. (green club-leaved): foliis imbricatis, in ramo-
3. rum apicibus rosulas subformantibus subclavatis, turgidis viridibus, apicem versus attenuatis acutis.

Habitat in Americâ Septentrionali.

Rami et *ramuli* perennes, primo anno 3–4-entales, stabiles, sed flexo-decumbentes teretes foliorum cicatricibus crebrè et transversaliter lineolatim notati, asperiusculi. *Folia* breviora et obtusiora quàm in affinibus hujus sectionis, sed lævia incurva tumida et carnosa; *subtùs* convexa, *suprà* obsoletè depressiuscula.

Floret nondum in Angliâ, sed in Horto Pharmac. Chelseiano pulchrè vigebat A.D. 1830. Species distinctissima, vix ullæ valdè affinis, sed prope *S. Forsteri*, *Engl. Bot. tab.* 1802, locarem pro tempore. H. ♃.

Obs. Under the genus *Sedum* may appropriately be noticed some *Seda* I have long cultivated, and which it will be a botanical improvement to describe briefly as follows; for the first three appear to be distinct species: viz.

Sectio OBTUSIFOLIA *Nob.* floribus cymosis.

album. S. (blunt-leaved white): foliis clavato-ovatis viridibus teretiusculis glabris, ramis perennibus radicantibus, junioribus in lente puberulis.

Sedum album *Eng. Bot. t.* 1578.

Sedum album γ *DeCand. Prod. v.* 3. *p.* 406.

Flores valdè cymosi albi.

Habitat insuper muros prope Londinum. H. ♃.

micranthum. S. (greater blunt-leaved): foliis clavato-oblongis viridibus teretiusculis glabris, ramis perennibus radicantibus, junioribus in lente puberulis.

Sedum album β micranthum, *DeCand. Prod. v.* 3. *p.* 406.

Habitat prope Gloster. H. ♃.

Communicavit amicus Rev. Dom. Ellicombe.

Obs. Antecedenti simillimum, at 2–3-plò majus, floribus numerosioribus minoribus petalis angustioribus.

tereti-

teretifolium. S. (slender blunt-leaved): foliis æqualiter teretiusculis subelongatis parùm depressis viridibus ramisque subelongatis perennibus radicantibus, omninò glaberrimis.

Sedum album *α*, *DeCand. Prod.* 3 *p.* 406. quod est *S. teretifolium Lam. Fl. Fr.* 3. *p.* 84.

Obs. I have not seen the flowers of this plant, which was sent to me four years ago, from the Bury Botanic Garden, as, I think, a Hertfordshire plant, gathered wild there by the late venerable Sir Thomas Cullum. Its shoots are more than twice the length of the last, and its leaves are half as long again, often cylindrical and more remote, and it is destitute of all pubescence even when magnified. I suspect it is a maritime plant, requiring a saline air, and perhaps a saline soil, to produce its flowers. It bears a greater resemblance to a *Mesembryanthemum* than any other *Sedum* I am acquainted with.

Habitat prope Hereford? H. ♃.

With the above *Seda*, mention may be here, and appropriately, made, of two remarkable varieties of *Sedum acre*, sent from the Bury Botanical Garden at the same time with the preceding; viz.

acre. S. var. *β diminutum.* multoties minus quàm *S. acre Eng. Bot. tab.* 839. An *Sedum acre β glaciale, DeCand. Prod. v.* 3. *p.* 407?

Obs. Valdè repens glomeratum, facie ferè *Lycopodii* parvi. *Erecti rami* steriles unciam alti; *florentes*, vix sesquiunciales. *Folia* magìs conferta et minùs viridia quàm in *S. acre Eng. Bot. tab.* 839, in cæteris ferè omninò concordat. Sed caules 2–3-, rariùs 4-flori solum, nec 4–8, flori ut in *S. acre.* H. ♃.

Obs. This diminutive plant, my judicious friend Mr. John Denson, jun. (late Curator of the Bury Botanical Garden) assures me was gathered wild on Swaffham Heath, Norfolk; by, I think, Mr. William Christie, jun. of Clapham; and it may eventually prove to be a genuine species.

The second variety may be called

acre γ elongatum. S. ramis pendulis 7-uncialibus; erectis 4-uncialibus, foliis laxè imbricantibus.

Obs. This var. Mr. Denson mentioned not the origin of. Its shoots are three times longer than those represented in *Eng. Bot. tab.* 839, and its leaves are more patulous and more distant from each other, and it pro-

duces flowers less frequently; but it is only a variety. H. ♃.

Genus ECHEVERIA *DeCand. l. c.*

Herbæ mexicanæ succulentæ glaucæ perennes vel suffrutescentes, *foliis* crassis lævibus basi solutis, et ferè *Sempervivi* modo in rosulas laxas expansis; *floribus* racemosis seu spicatis, vel spicatim paniculatis coccineis seu luteis, pedunculis brevibus seu brevissimis.

* *Floribus coccineis.*

lurida. E. (The dingy-leaved) subcæspitosa: foliis imis lan-
4. ceolato-cuneatis lividis, superioribus lanceolatis, floribus racemoso-spicatis.

Habitat in Mexico. G. H. ♃.

Communicavit amicus Dom. Otto.

Obs. Herba succulenta perennis sive suffrutex laxè cæspitosa surculis paucis brevibus. *Folia* paginis lævibus glauco-fusca inflexo-concava, et nitentia, si benè culta; *subtùs* convexa obtusè carinata, margine cartilagineo, tenuiter exasperato; *apice* sæpè parùm recurva acuminulata. *Scapi* in plantâ nostrâ tres dodrantales vel pedales, teretes et minimè sursùm attenuati læves pallidi seu albicantes, primò terminales, sed citiùs è progressione plantæ laterales et basi parùm flexi, mox recti et erecti foliolosi seu bracteati. *Bracteæ* alternæ, folio consimiles, gradatim sursùm minores, distantes patulæ, vel inter flores recurvulæ. Omnes *bracteæ* sic leviter scapo adnatæ, ut citiùs deciduæ, e levissimo tactu, et insuper terram facillimè radicantes, et (uti folia) in proprias plantas crescentes. *Flores* racemosi, seu racemoso-spicati; superiores confertiores; aperti horizontales. *Pedunculi* breves subindè 1–2-bracteolati. *Corolla* ut in *E. grandifolia Nob.* sed magìs coccinea, et certo situ rore glauco cum rubedine vivaciter violascens.

Ord. Nat. EUPHORBIACEÆ, *Juss. Gen.* 384.

Genus TITHYMALUS *Mill.*, *Nob. in Synops. Pl. Succ. p.* 137.

uniflorus. T. (American tuberous) tuberosus lævis: foliis obo-
5. vatis subpetiolatis, floribus pedicellatis solitariis in ramorum dichotomiis.

Habitat in Americâ Meridionali. St. ♃.

Florebat in caldario regio ditissimo Horto Kewensi A.D. 1829, mense Julii, ubi descriptionem sequentem faciebam.

Radix

Radix ferè ut in *Tith. tuberoso Nob.* qui est è Capite Bonæ Spei, et satìs cognitus. *Caules* pauculi subsemipedales exigui teretes patentes carnosuli herbacei, in caldario debiliter decumbentes furcati sive simpliciter dichotomi, apicibus assurgentibus.

Folia alterna remotiuscula integerrima in petiolos (in caldario) deorsùm attenuata subavenia pallidè viridia satìs tenuia, nihilominùs carnosula. *Flores* pauci parvi ordinarii inconspicui.

Cætera non examinavi, et post florescentiam perfectam solùm vidi. Est planta herbacea tuberosa exigua lævis edentula et adhuc non scripta.

Ord. Nat. CACTEÆ, *DeCand. Prod. v.* 3. *p.* 457.

Tribus primus; seminibus parietalibus.

Subgenus Echinocactus *Link et Otto Diss. p.* 11.

Cotyledones duæ basilares (secund. DeCand.). *Inflorescentia* apicem plantæ versus. *Axis centralis* lignosus *Cereorum* modo.

Obs. Plantæ multangulares breves carnosæ epidermide duro lævi tectæ suboblongæ grossæ vel sæpissimè spheroideæ; *costis* spinis validis sæpè intertextis crebrè armatis. *Subgenus* solum *Cereo* proximum.

Sectio nova Gibbosi. *Costis* gibbere notabili carnoso subsingulo spinario*, præcipuè in junioribus notatis, inde (costæ) supernè lobulatim undulatæ evadunt.

Huc referendi *Ech. gibbosus* necnon *E. nobilis Nob.*

subgibbosus. E. (strong-spined gibbous) rotundo-oblongus, in-
6. tertextim valdè spinosus; angulis subsexdecim lobularibus, sinubusque profundis acutis, spinariis distantiusculis.

Habitat prope Valparaiso in Americâ Australi, ubi legit Dom. Brydges, misitque Dom. Tate vico Sloane-street, qui mecum communicavit mense Junii A.D. 1830. St. ♄.

Obs. Planta adhuc nondum floruit apud nos: sim-

* *Spinarium* in *Cacteis* est organum corneum induratissimum carne immersum in quo *spinæ* distinctè insertæ sunt, uti animalium dentes in maxillam.

Spinaria foliorum forsan modificationes sunt, et rami, ramuli, floresque, necnon subindè folia vera, vel foliola, ex eorum axillis prodeunt. Nihilominùs *squamulæ* forsan *foliaceæ* exstant in *Cereo squamuloso,* aliisque perpaucis; *foliola*que sediformia vera in numerosis *Opuntiis,* omninò *extra* spinaria. Supradictæ squamulæ *Sectionem* novam indicant; uti *aphyllæ Opuntiæ.*

plex

plex est, 5 unc. alt. et 3 unc. lat. epidermide lætè viridi, apice depresso umbilicato. Angulæ validæ spinis horridis intertextis totam plantam ferè tegentibus. *Spinarium* sub-16-spinosum lanosum hemisphæricum. *Spinæ* sub-12–13, rectæ; harum imæ ferè æquales setiformes subsemunciales albicantes seu pallidæ radianter horizontales: sub-sex *summæ* subaculeiformes multoties majores subunciales validiores basi bulbosæ patulæ fulvicantes apicibus rufescentibus: harum ultimarum (spinarum) una cæteris respectu centralis et erectus est.

Genus CEREUS *Mill.*, *Nob.*

Sectio GRANDANGULARES, erecti, *Nob.*

validus. C. (stout quadrangular) tetragonus firmus: apice
7. glauco; lateribus ferè planis, vel primo convexiusculis; ipsis angulis obtusissimis, mediocriter spinosis.

Habitat in Americâ calidiore. St. ♄.

Obs. Unam plantam nativam in caldario regio horto Kewense pulchrè crescentem solùm vidi. Nunc in quarto anno simplex et subsesquipedalis est, erectus, basi trigonus, supernè tetragonus; facie et crassitie ferè *C. tetragoni Linn.* sed affinior fortè *C. obtuso Nob.*; at valdè robustior. *Spinaria* inter se valdè distincta, *lana* ordinaria densissima centrali brevi; *spinis* variis mediocribus erecto-patulis fulvo-fuscis, apicem versus sæpè fulvicantibus et 3–6-linearibus.

Ante *Cereum obtusum Nob.* certè locarem.

Obs.—Cereus magnus Nob. in *Phil. Mag.* Feb. 1830, *p.* 109: bis florebat in caldario apud Dom. Rolls, King's Road, Chelsea, in annis 1830 et 1831, Augusti mense.

Descriptio:—Flores diurni speciosi solitarii, apicem versus suberecti dodrantales et ferè ut in *C. hexagono* parùm suaveolentes. *Tubus* cylindraceus usque ad medium æquali crassitie virescens, et affinium more squamulosus et spinulosus, spinis ibi longioribus gracilioribus; *supra medium* duplò inflatior pallidior, *squamulis ordinariis calycinis* lineari-acuminatis distantibus viridibus; tunc *petaloideis* et *petalis* etiam ordinariis albis lorato-lanceolatis acutis, toto mane apertis patulis usque ad 4 uncias, *subtùs apicem* parùm graciliter carinulatis. *Stamina* numerosissima pallidè sulphurascentia, in ordine conferto regulari circulari juxta petala, sed eos non tangentia, antheris minutis. *Stylus* ordinarius parùm declinatus, altitudine circiter staminum, *stigmatibus* 14 radianter

radianter recurvulis obtusis subquadrilinearibus aliquantillum puberulis. *Cætera* ordinaria.

Ord. Nat. FICOIDEÆ, *Juss. Gen.* 315.

Genus MESEMBRYANTHEMUM *Linn. Gen.* No. 628.

Sectio PLATYPHYLLA.

†† Foliis planioribus vix undulatis basi cuneatis.

puberulum. M. (white-flowered puberulose): foliis oppositis
8. alternisque obovato-spathulatis inflexo-canaliculatis carinatis; ramis floriferis, foliorumque marginibus pubescentibus: pedunculis subcylindricis erectis.

Obs. A seminibus nativis Capensibus in horto Chelseiano ortum A.D. 1829. G. H. ♂.

Radix annuus fibrosus. *Caulis* ramosus procumbens; *ramulis* erectis subteretibus vel paululum angulosis crystallino-papulosis ad lucem uti folia calycesque. *Folia* et *Calyx* ferè ut in *M. papuloso Linn.* cui forsan proximum, à quo differt in pedunculis non reflexis. *Flores* ferè ut in *M. papuloso*, pomeridiani semunciales albi, (*pedunculis* uncialibus) imprimis apice, extùs flaveolentes sed mox undique albi, *petalis* linearibus obtusis. *Genitalia* flava. *Styli* 5, apicibus patulis seu valdè recurvis; *stigmatibus* capitularibus, *antheras* pallidioribus et plerumque superantibus.

Obs. Pone *M. papulosum Linn.* locarem, cui simillimum, sed staturâ adhuc breviore foliis floribusque, etc., eadem magnitudine. *Calyx* 4–5-phyllus, basi 4–5-gonus, *foliolis* 2–3, foliiformibus, at longè minoribus; 2 aliis valdè diminutis et alatim membranaceis. *Cætera* nor examinavi.

Sectio nova, STENA. Suffrutices ferè minimi, *ramis* subindè semipedalibus effusis, *foliis*que filiformibus, *petalis* angustissimis vel setaceis pallidissimè roseis. Pone *sectionem* PALLIDIFLORAM locarem, cum *M. debili Nob.* in *Phil. Mag.* pone *M. stenum*, cui magis affine quàm antiquo *M. reptanti.*

stenum. M. (The slender): ramis effuso-decumbentibus flexu-
9. osis filiformibus, foliis gracilibus falcato-incurvulis triquetro-teretiusculis mucronatis paucipunctatis glaucescentibus, floribus 1–3-natis terminalibus.

Habitat C. B. S. G. H. ♄.

Obs. Vigebat in regio horto Kewense A.D. 1830 è seminibus nativis à Dom. Bowie missis. *Florebat* Aug. 1831. Suffrutex tenuis, ramis semipedalibus subconfertis

fertis numerosis sæpiùs decumbentibus et effusis, aëre aperto cortice basin versus albicante, medio, rufo-fusco sive badio, apice purpurascente. *Folia* 5-9-linearia lineam lata medio subcompressa, utraque subattenuata glauca seu glaucescentia ad lucem sæpiùs obsoletè paucipunctata. *Pedunculi* breves teretes clavati. *Calyx* 5-phyllus ordinarius subtuberculatus foliolis acuminatim mucronulatis. *Corolla* antemeridiana pallida (*M. glomerato* major) violaceo-erubescens, *petalis* valdè angustatis integris, interioribus pedetentim minoribus tenuioribus pallidioribus s. albis setiformibus et magis magisque imperfectè antheriferis, et erecto-collectis usque ad vera *stamina*, quæ petalis breviora sunt et collecta, *antheris* pallidè sulphurascentibus. *Styli* 5 breves inter stamina reconditi ordinarii.

Obs.—M. debile Nob. (quod affinior *M. steno* quàm *M. reptante*) florebat in regio horto Kewense, A.D. 1830, verno tempore. *Flores* terminales perpauci, minores quàm in *M. steno*, pallidissimè erubescentes sive albicantes, antemeridiani. Vidi sed non examinavi.

Sectio HISPICAULIA *Nob.*

furfureum. M. (branny-twigged): ramis confertis rectiusculis
10. rigidis furfuraceis, foliis cylindricis obtusissimis calycibusque obsoletiùs papuloso-crystallinis; floribus parvulis numerosis.

Habitat Cap. Bon. Spei, ubi invenit spontaneum misitque ad regium hortum Kewensem A.D. 1830 amicus Dom. Bowie. G. H. ♄.

Obs. Nova species. Suffrutex dumosus dodrantalis rigidus. *Rami* seu *ramuli* recti numerosi patuli et ferè filiformes furfuro at oculo inarmato non valdè conspicuo tecti. *Folia* matura in florentibus plantis aëre aperto conferta, 4–9-linearia subpallidè seu sordidè virescentia patentia in ramulis numerosis brevissimis lateralibus et copiosè florigeris. *Flores* ramulos terminantes sæpè solitarii, pedunculis brevibus furfuraceis absque *bractæis* propriis. *Calyx* 5-phylloideus ordinarius papuloso-crystallinus, foliolis obtusissimis subæqualibus longitudine. *Corolla* inter minores, lætè rubicunda subsemiuncialis A.M. expansa sub radiis solaribus, *petalis* uniserialibus sublinearibus sæpiùs integris.

β. Variat, minus: floribus sæpè ternatis.

Obs.—M. floribundo Nob. affine, a quo differt staturâ subduplò

subduplò minore ramis rigidioribus fortè confertioribus et rectis vel subrectis (non torquatis ut in *M. floribundo*) ramulis patulis; foliis forsan minùs viridibu s; at nihilominùs pone id locarem.

Obs. Folia summa incipientia sæpè obsoletè trigono-cylindracea, sive suprà planiora vel depressa.

Obs. Under this Section of the present Genus it is appropriate to remark that *M. hispidum* α & β of *Revis. Pl. Succ. p.* 186. are two distinct species, easily separated by the following characters.

hispidum. M. (hispid-peduncled): foliis cylindricis obtusissimis calyceque glabro obconico viridibus papuloso-micantibus, staminibus pistillo longioribus, ramis pedunculisque hispidis.—*M. hispidum* α, l. c.

subhispidum. M. (smooth-peduncled): foliis cylindricis obtusissimis calyceque glabro ferè obconico subviridibus papuloso-micantibus, ramis plerumque et pedunculis omninò depilatis.—*M. hispidum* β, l. c.

Obs. Priore elatius, longè minùs ramosum, *ramis* longioribus erectioribus strictioribus, distantioribus gracilioribus et florentibus ferè semper denudatis vel aliquantillum furfuraceis. *Folia* (uti calyces) minùs papuloso-micantia, et minùs intensè viridia. *Petala* duplò latiora seu minùs cuneata, longè pallidiora, costâ basi subindè albicante, apice altiùs emarginatâ. *Stamina* stylos subæquantia, pallidiora breviora minùs expansa, at crassiora, uti *antheræ* itidem pallidiores. *Capsulæ* angulis maturis, longè minùs productis, et obtusioribus.

Obs. Professor DeCandolle, in his *Prod. v.* 3. *p.* 441, under his *M. tuberculatum*, cites my *M. hispifolium* as synonymous, saying "foliis acutis papulosis mollibus" &c.—but my plant in its *var.* α, has folia valdè obtusa pilis respicientibus tecta: and in its *var.* β, which is twice as large, is rather less obtuse-leaved, but by no means acute. Can M. DeCandolle's plant, therefore, be my *M. hispifolium* β? or, are the three distinct? My β. I have known forty years in Chelsea Garden; α. only seventeen years.

Obs. 2. Closely allied to this species may appropriately be briefly added, for the first time, an account of the rare flowers of *M. candens, Nob. Revis. Pl. Succ.* 186. and of a new variety of it from the Prince of Salm-Dyck, as follows:

candens.

candens. M. (white trailing hispid):

α minus: foliis magìs candentibus. *Flores* (duos solùm vidi) terminales in ramulis lateralibus, solitarii parvi albi concinni antemeridiano, mox erubescentes, denique lætè rubicundi. Cæteri ordinarii.

Floret Junii mense. G. H. ♄.

β viridius, duplò majus omni parte, foliis viridioribus floribus (horum 2 solùm vidi) albis seu niveis pulchrioribus mox erubescentibus, denique altè rubicundis, antemeridianis.

Floret Septembris mense. G. H. ♄.

Obs. 3. *Rami* prostrati hujus speciei ex eorum nodis valdè radicant, uti *Fragrariæ* sarmenti.

LV. *Additional Remarks on Isomorphism. By* H. J. Brooke, *Esq. F.R.S. L.S. & G.S.**

IN the appendix to a work just published by Dr. Daubeny on the Atomic Theory, some observations are inserted relative to a paper of mine on Isomorphism, inserted in No. 57 of this Journal.

Upon these observations I shall at present only remark, that they contain two paragraphs upon which I am desirous of immediately setting myself right with Dr. Daubeny and the readers of his work.

Dr. Daubeny says, "It seems impossible to apply the [theory of mixture of foreign matter] to the garnet species, neither has Mr. Brooke attempted to do so."

What mineral is it, I should first be disposed to inquire, that is to be designated by the name of Garnet? It is not denoted by its crystalline form, for that is common to a variety of others; and there is no very apparent reason why, among the substances which, chiefly on account of their similar forms, have been included under the supposed garnet tribe, there should not be some, which, on account of their chemical differences, do really constitute distinct species. To enter, however, upon an investigation of this question would require a knowledge of the chemical constitution of the matrices or rocks in which the several varieties have been found;—a point upon which scarcely any correct information can be collected from the published works on the subject. And probably on reconsideration Dr. Daubeny will be of opinion, that the garnets are, from the class of forms to which

* Communicated by the Author.

they

they belong, the last which ought to be adduced as evidence either of the truth or fallacy of isomorphism.

The second paragraph to which I have alluded is as follows:—

"It may be remarked, that in the case of Stilbite *which Mr. Brooke has brought forward as an exception*, he has overlooked the presence in it of 6 atoms of water,—a circumstance which constitutes a chemical difference between that mineral and paranthine."

This however involves an entire misrepresentation of what I have stated;—unintentional I have no doubt, and occasioned by hasty perusal of what I wrote. I did not, as Dr. Daubeny will perceive on looking again at my paper, raise any question about the *form* of *Stilbite*. I referred to *Eudyalite* as an example in which *soda* and *lime* are said to be isomorphous in respect of 1 *atom* of *silica;* to *Paranthine*, in which they are said to be isomorphous in respect of 2 *atoms* of *silica;* and to *Stilbite*, in which they stand in the same relation to 3 *atoms* of *silica*. And I did this merely to adduce the inference that all other elements which might be supposed isomorphous in respect of 1 atom of silica, ought to be equally so when combined with 2 atoms, or with 3 atoms; and hence it was unnecessary even to consider the water or any of the other component elements of either of these minerals.

With regard to *plesiomorphism*, it does not appear that any limit has been assigned to the difference in the plesiomorphous angles. *Silica* and *alumina* are said to be isomorphous *when alumina acts the part of an acid*. But as the question raised is, whether the *atoms* of *silica* and *alumina* are isomorphous,—unless those of *alumina* differ according as this substance acts the part of *acid* or of *base*, and in this case it is not easy to conceive what an *atom* means,—*silica* and *alumina* should be *isomorphous* generally. Now the primary form of silica, or quartz, is a *rhomboid* of 94° 15′, and that of alumina, or corundum, also a *rhomboid*, but measuring 86° 5′. Hence these bodies are, when they occur singly, only plesiomorphous, with a difference in their angles of 8° 10′, notwithstanding which they are required by the theory to pass for strictly isomorphous elements when alumina becomes a substitute for silica. When however they enter into mutual combination in *cyanite*, which is regarded as a pure silicate of alumina, both isomorphism and plesiomorphism at once vanish, and a *new class* even of form is produced, a *doubly oblique prism*, as irregular and remote a form from the *rhomboid* as can be found amongst crystals.

How these facts are to be reconciled with the new theory, I must leave to the better consideration of its supporters.

 LVI. *Notices*

LVI. *Notices respecting New Books.*

The Life of Sir Humphry Davy, *Bart. LL.D. late President of the Royal Society, Foreign Associate of the Royal Institute of France, &c. &c. By* John Ayrton Paris, *M.D. Cantab. F.R.S. &c. Fellow of the Royal College of Physicians.*

[Concluded from page 386.]

AFTER completing the history of the Safety-lamp, Dr. Paris briefly notices the Geological Society of Cornwall, and the patronage and support which it received from Sir H. Davy: to this Society he communicated a memoir on the Geology of Cornwall, which was published in the first volume of its Transactions. Of this paper Dr. Paris gives an analysis, and then proceeds to exhibit Sir Humphry in quite a new field of inquiry,—that of discovering a method for unrolling the ancient Papyri. "It occurred to him," says Dr. Paris, "that as chlorine and iodine do not exert any action upon pure carbonaceous substances, while they possess a strong attraction for hydrogen, these bodies might probably be applied with success for the purpose of destroying the adhesive matter, without the possibility of injuring the letters of the Papyri, the ink of the ancients, as it is well known, being composed of charcoal. He accordingly exposed a fragment of a brown manuscript, in which the layers were strongly adherent, to an atmosphere of chlorine; there was an immediate action, the papyrus smoked, and became yellow, and the letters appeared much more distinct. After which, by the application of heat, the layers separated from each other, and fumes of muriatic acid were evolved. The vapour of iodine had a less distinct, but still a very sensible action. By the simple application of heat to a fragment in a close vessel filled with carbonic acid, or with the vapour of æther, so regulated as to raise the temperature very gradually, and as gradually to reduce it, there was a marked improvement in the texture of the papyrus, and its leaves were more easily unrolled. In all these preliminary trials, however, he found that the success of the experiment absolutely depended upon the nicety with which the temperature was regulated."

So great indeed was the care required in performing this operation, that the plan proposed was incapable of application to any useful extent; indeed, the condition of the Papyri was such, that it would have been sufficient to damp the ardour and paralyze the exertion of any one less accustomed to combat and conquer difficulties than was Davy. An account of the various processes may be found in his paper, entitled, "Some Observations and Experiments on the Papyri found in the Ruins of Herculaneum," which was read before the Royal Society on the 15th of March 1821, and published in the Transactions for that year*.

Although the liquefaction of the gases is a subject more connected with the name of Faraday than with that of Davy, we cannot resist the temptation to insert the following statement from Dr. Paris: "Every

* This paper will also be found in the Phil. Mag. vol. lviii. p. 421.

incident,

incident, however trifling, if it relates to a great scientific discovery, merits the attention of the historian. As it accidentally occurred to me, and to me alone, to witness the original experiment by which Mr. Faraday first condensed chlorine gas into a liquid, I shall here state the circumstances under which its liquefaction was effected.

"I had been invited to dine with Sir Humphry Davy, on Wednesday the 5th of March 1823, for the purpose of meeting the Rev. Uriah Tonkin, the heir of his early friend and benefactor of that name. On quitting my house for that purpose, I perceived that I had time to spare, and I accordingly called in my way at the Royal Institution. Upon descending into the laboratory, I found Mr. Faraday engaged in experiments on chlorine and its hydrate in closed tubes. It appeared to me that the tube in which he was operating upon this substance contained some oily matter, and I rallied him upon the carelessness of employing soiled vessels. Mr. Faraday, upon inspecting the tube, acknowledged the justness of my remark, and expressed his surprise at the circumstance. In consequence of which, he immediately proceeded to file off the sealed end; when, to our great astonishment, the contents suddenly exploded, and the oily matter vanished!

"Mr. Faraday was completely at a loss to explain the occurrence, and proceeded to repeat the experiment with a view to its elucidation. I was unable, however, to remain and witness the result.

"Upon mentioning the circumstance to Sir Humphry Davy after dinner, he appeared much surprised; and after a few moments of apparent abstraction, he said, 'I shall inquire about this experiment to-morrow.'

"Early on the next morning, I received from Mr. Faraday the following laconic note:

"'Dear Sir,

'The *oil* you noticed yesterday turns out to be liquid chlorine.

'Yours faithfully, 'M. FARADAY.'

"It is well known that, before the year 1810, the solid substance obtained by exposing chlorine, as usually procured, to a low temperature, was considered as the gas itself reduced into that form: Sir Humphry Davy, however, corrected this error, and first showed it to be a hydrate, the pure gas not being condensable even at a temperature of $-40°$ Fahrenheit.

"Mr. Faraday had taken advantage of the cold season to procure crystals of this hydrate, and was proceeding in its analysis*, when Sir Humphry Davy suggested to him the expediency of observing what would happen if it were heated in a close vessel; but this suggestion was made in consequence of the inspection of results already obtained by Mr. Faraday, and which must have led him to the experiment in question, had he never communicated with Sir Humphry Davy upon the subject. This avowal is honestly due to Mr. Faraday."

* The results are contained in a short paper in the Quarterly Journal of Science, vol. xv.

After

After the numerous and long extracts which we have made from Dr. Paris's work, we can allot but little space for further remarks, though there are various subjects of high interest upon which we have not bestowed a single observation; they are noticed by the biographer with a minuteness and an accuracy worthy of the illustrious philosopher whose discoveries he has undertaken to record. For an account of the share which Davy had in developing the nature and properties of iodine; of his method of protecting copper sheathing; his experiments on electro-magnetism, and the paper on the phænomena of volcanoes, we must refer the reader either to the memoirs themselves*, or to Dr. Paris's account of them. Alluding to his retirement from the office of President of the Royal Society, Dr. Paris remarks: "To assert that Davy retained his popularity, or to deny that he retired from the office under the frown of a considerable party, would be dishonest. I would willingly dismiss this part of his life without too nice an examination; but I am writing a history, not an eloge.

"As a philosopher, his claims to admiration and respect were allowed in all their latitude; but when he sought for the homage due to patrician distinction, they were denied with indignation. How strange it is, that those whom Nature has placed above their fellow men by the god-like gift of genius, should seek from their inferiors those distinctions which are generally the rewards of fortune. When we learn that Congreve, in his interview with Voltaire, prided himself upon his fashion rather than upon his wit; that Byron was more vain of his heraldry than of his 'Pilgrimage of Childe Harold;' that Racine pined into an atrophy, because the monarch passed him without a recognition in the ante-room of the palace, and that Davy sighed for patrician distinction in the chair of Newton, we can only lament the weakness from which the choicest spirits of our nature are not exempt. Will philosophers never feel, with Walpole, that 'a genius transmits more honour by blood than he can receive'? Had the blood of forty generations of nobility flowed in the veins of Davy, would his name have commanded higher homage, or his discoveries have excited greater admiration? But great minds have ever had their points of weakness: an inordinate admiration of hereditary rank was the cardinal deformity of Davy's character; it was the centre from which all his defects radiated, and continually placed him in false positions; for the man who rests his claims upon doubtful or ill-defined pretensions, from a sense of his insecurity, naturally becomes jealous at every apparent inattention, and he is suspicious of the sincerity of that respect which he feels may be the fruit of usurpation. If with these circumstances we take into consideration the existence of a natural timidity of character, which he sought to conquer by efforts that betrayed him into awkwardness of manner, and combine with it an irritability of temperament which occasionally called up expressions of ill-humour, we at once possess a clue by which we may unravel the conduct of our philosopher,

* See also Phil. Mag. vol. lviii. p. 43, 406; lxiv. p. 30; lxv. p. 203; and Phil. Mag. and Annals, N.S. vol. iv. p. 85.

and

and the consequences it brought upon himself during his presidency of the Royal Society."

We quote this last paragraph not merely on account of its intrinsic truth, but to show that while Dr. Paris fully appreciated the merits of the philosopher, as is proved indeed by the ability with which he has described his discoveries, he was fully aware of the weak points of his character. We should also be happy to copy Dr. Paris's very able comparison of the genius of Wollaston with that of Davy, did our limits allow.—We conclude with acknowledging the great pleasure with which we have read Dr. Paris's work, and with strongly recommending it to the perusal of all who delight in tracing the progress of genius successfully applied to the achievement of objects whose value is imperishable.

Montagu's Ornithological Dictionary; new edition, "with a Plan of Study, and many new Articles and original Observations. By James Rennie, *A.M., A.L.S., Professor of Natural History, King's College, London; Author of 'Insect Architecture,' 'Insect Transformations,' 'Architecture of Birds,' &c."* London, 1831. Octavo; Introduction, &c. pp. lx., Dictionary and Index, pp. 592; 28 Engravings on Wood.

[Continued from p. 379.]

We may here usefully make a few observations on the distinction between what is properly called the Natural System and Artificial Systems; as much needless controversy (in our opinion) has ensued upon this particular subject, to which we have already alluded in p. 376. An artificial system, then, as we conceive, is merely an arrangement of species, according to some particular character or characters selected for the purpose, into such groups, of greater or less magnitude, as will enable the naturalist to recognize any species which he may discover or observe, and by ascertaining its place and denomination in the system, obtain a clue to direct him to all the sources of information respecting it, and thus to become acquainted with its importance and value, whether as an object of philosophical investigation, or as one of utility to mankind; or if the species be new (which point it will also determine), it will enable him to define it, and give it its proper place in the system, for future reference. Of such a system, and of the use of such a system, the Linnæan *Systema Plantarum,* in which the beings of the vegetable kingdom are thus eclectically arranged, according to the characters of the fructification, is an eminent example.

The Natural System, *e contra,* is the distribution of species into a series, according to the mutual relations subsisting between them, ascertained by observing the totality of their characters; not by selecting any individual character, or individual set of characters, to serve as the basis of classification. And whether the visible exponent of this system be a series of specimens of the species themselves, a linear representation of it in a diagram, or a verbal expression of it in a book, it is still, in either shape, as a *branch of science,* the Natural System; being, as we have before observed, all that man can know or

or possess of the system actually existing in the creation. Of this system, we cite the arrangement of the Animal Kingdom, and especially of the *Annulosa*, given in Mr. Macleay's *Horæ Entomologicæ*, as being the first approximation to, with respect to the Animal Kingdom generally; and Mr. Vigors's arrangement of Birds, Mr. Macleay's arrangement of the *Coleoptera Chilopodomorpha* of Java, and Dr. Horsfield's of the *Lepidoptera Diurna* of the same island and other parts of India, together with some other essays, as other approximations to, in different parts of the animal series. The Jussieuian system of plants, so far as the distribution of species into what are called natural orders is concerned, and certain essays of Mr. R. Brown, especially his *Prodromus Floræ Novæ Hollandiæ*, are also examples of approximations to it, as existing in the Vegetable Kingdom; though in many respects, we apprehend, of a more remote nature than the former. While the *Règne Animal* of Cuvier, we conceive, presents a mixture of artificial systems on various plans, with portions of the true natural system, detected by the sagacity of its illustrious author, and by that of his predecessors and his contemporaries.

But Mr. Macleay has himself furnished us with an illustration of the distinction between an artificial system and the natural system, which on account of its clearness and effect we will now give in his own words.

"Were the planets to be arranged in a table according to any one of their properties,—as for instance, the period of rotation on their several axes,—such a system would be artificial, and only useful in that, having observed the length of a rotation, a reference to the table would be a convenient mode of determining the name of the planet. But no one would ever think of confounding this artificial table or system with the system of the universe [meaning thereby our solar system]; although an error exactly similar is every day committed in natural history, when a person who may by the mere exercise of his memory have become acquainted with an artificial table, fancies that he must therefore be a profound naturalist."—*Hor. Ent.* pref. p. xi.

Now we think the above a very just illustration of the subject; the supposed table of the planets arranged according to their respective periods of rotation, being an exact exemplification of an artificial system in natural history; and a graphic representation of the actually observed arrangement of them about the sun, or a verbal description or other expression of it in a book, being the same thing, with respect to them, that the natural system, or an approximation to it, is, with respect to the species of animals or of plants; with the exception, that, in the case of the planets, the natural system actually exists (as a system) in *space*, but that in the case of the species of animals and plants, *it is not exhibited in space;* but is the result of induction from particular instances, each particular instance itself consisting of a group of animals or of plants previously ascertained, by observation and that spontaneous induction which is almost coincident with it, to be natural assemblages.

This mode of considering the subject leads us to another point which appears to be of some importance, and which will probably tend to show the true relation of the mode of investigating nature introduced into

into natural history by Mr. Macleay and his disciples, to that which has long been pursued in dynamics, astronomy, and other departments of general physics. This is a point on which we feel some anxiety, for we are of opinion that Mr. Macleay's discoveries, and they alone, have at length raised the study of Natural History to a level with the higher Physics. And as the cultivators of the latter do not as yet appear adequately to appreciate the dignity, as a branch of inductive science, of Natural History, we are on that account also glad to have this opportunity of drawing their attention to the subject, by stating what we believe to be the correct view of it in this respect.

The assembling of species into groups, then, according to the totality of the characters of every species, and the variation of those characters, and of the groups so established into greater groups, by the same process, is, we conceive, precisely analogous to what Mr. Herschel, in his Preliminary Discourse on the Study of Natural Philosophy, terms "the first stage of induction" that is, the discovery of proximate causes, and laws of the lowest degree of generality, in physics. And the investigation of the abstract principles of affinity and analogy; of the true nature of what Mr. Macleay has called osculant groups; of the intimate quality of the affinity of transultation; of that of the process by which, in the union of contiguous groups of a certain rank, the relation of analogy insensibly becomes a relation of affinity, &c., are as precisely analogous to the higher degrees of inductive generalization, in physics. With the above, other considerations are connected, which may throw some degree of light on the misconceptions which have been broached on the subject we have already twice noticed, the true claims of the Macleayan system to be that of nature, and which are in a manner sneered at by Mr. Rennie, in his introductory page examined in p. 375—379.

We will return to Mr. Macleay's illustration of the difference between an artificial system and that of nature, by allusion to a supposed and to the real arrangement of the planets composing our solar system. Pursuing this illustration, we affirm, that the discovery of the natural system by the means adopted by Mr. Macleay and the naturalists of his school, is closely analogous to, is as strictly inductive, as truly logical, and claims as much the regard of every reflecting mind, as, the discovery of the *natural system* of the planetary and sidereal universe, by the means, appropriate to that sphere of research, which have been adopted by astronomers and mathematicians. For the further use of this illustration we will extend it; we will regard the *natural system* of the planets as being, not merely their arrangement, as circulating around the sun, and the knowledge of it, but as the assemblage of laws which govern their motions, and of proximate or remote causes of their phænomena.

The *natural system* of the planets, taking that term in its lowest acceptation, was, as is well known, discovered by Copernicus. But his discovery, although complete in itself, although consisting of demonstrated truths, did not extend to the entire natural system of these bodies; although the system promulgated was, in the proper sense of the term, a *natural* one, yet much remained to be discovered of that

system

system. Accordingly the system was extended by Kepler's discovery of the proportions between the times of revolution of the planets and their distances from the sun. Again, after Kepler had promulgated his discoveries, Galileo discovered the satellites of Jupiter, and by observations on these, Kepler saw it ascertained that the law which he had discovered to apply to the revolutions of the planets around the sun, held good also when applied to the periods of circulation of the satellites of Jupiter around that planet; "thus demonstrating it to be something more than a mere empirical rule, and to depend on the intimate nature of planetary motion itself."* But it was left for the genius of Newton to complete, to a certain degree of generality, the natural system of the planets which had been discovered by his predecessors. It was shown by him, in the Principia, that all the celestial motions which had down to his time been made known, were consequences of one simple law of attraction. And the consequences of this law, called that of gravitation, have been since pursued, through all their intricacies, by Clairaut, D'Alembert, Euler, Lagrange, and Laplace; and they are still pursuing by the successors of those philosophers,—by Ivory, Herschel, Airy, Lubbock, Poisson, and many others.

Now all this, we observe, is nothing but a series of investigations having for their object the discovery of what, in Astronomy, exactly answers to the natural system (now using that term in the highest and most comprehensive sense) in Natural History; and each contributor, whether practical observer or mathematician, actually has discovered *some part* of that system,—some portion of what was included in the general natural system discovered by Copernicus, which remained unknown until the time of the particular discoverer. It will of course be remembered that we are comparing the progress made in Astronomy with that of Natural History, here, merely for the purpose of illustrating what we conceive to be the legitimate object of the investigations of both, the discovery of *the natural system;* and that the two sciences have been regarded as comparable, *mutatis mutandis* only. And in the further comparison which follows, we beg it to be observed that we are not in any manner intending to make out a parallel, as to genius or scientific character, between the philosophers we have named as the successive discoverers of the natural system in astronomy with the naturalists we are about to name, as, among others, the discoverers of the natural system in Zoology. We presume not to offer any opinion on this point.

One of the reasons which have induced us to enter upon this detail of illustration, is the idea which has often been expressed, both orally at various scientific meetings, and also in print, that the discovery of the natural system is a thing which cannot or ought not to be aimed at—that it is not a legitimate object of science. Our opinion being diametrically opposite to this, we wish to show that what Mr. Macleay and the naturalists of his school are endeavouring to accomplish for Natural History, exactly corresponds to what Copernicus, Kepler, Newton, and Laplace have accomplished in part for Astronomy,

* Herschel's Preliminary Discourse, p. 269.

nomy, and what Ivory, and Lubbock, and Poisson, &c., are still contributing fresh truths to. They aim at nothing more; but they aim at nothing less.

The natural distribution of the Animal Kingdom then, as given in Mr. Macleay's *Horæ Entomologicæ,* may be compared, in a general manner, with the natural system of the Planets, as discovered, equally by the method of induction, by Copernicus; and the additions to and extensions of that distribution, which Mr. Vigors has produced with respect to Birds, Dr. Horsfield with respect to Lepidopterous Insects, &c., may be compared with the further unfolding of the planetary system by the labours of the successors of Copernicus. We do not presume to assert that the labours of these latter naturalists are of *equal degree* or *rank,* as contributions to Natural History, with the discoveries of Kepler and Galileo, as contributions to Astronomy;—we give no opinion on this point—but we affirm that they have extended the knowledge of the natural system of animated nature, communicated by Mr. Macleay, *in like manner* as Kepler and Galileo extended that communicated by Copernicus of the natural system of the planets. We certainly are of opinion, however, that Mr. Macleay's discovery of the natural system in Zoology is of similar importance as a contribution to that science, to what the discovery by Copernicus of the natural system of the planets was as a contribution to Astronomy; and that it will lead to results, in the researches of the naturalists of future ages, as important as those which Kepler, Galileo, Newton, and their successors have effected in the development of the particular "natural system" which formed their object of pursuit.

Sept. 29, 1831. [To be continued.]

LVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Nov. 2.—THE Society assembled this evening for the Session.

A paper was read, "On certain younger deposits in Sicily, and on the phænomena accompanying their elevation." By Dr. Turnbull Christie, F.G.S., and communicated by the President.

The observations contained in this essay were made partly during a short visit to Palermo, and partly on an excursion in which the author travelled from Palermo along the northern coast as far as the Castello di Tusa, crossed the central chain of mountains by way of Mistretta and the Monte di Castelli to Nicosia, Leonforte, and Castro-Giovanni, turned eastward by way of San Filippo d'Argire to Catania, and then proceeded along the east coast by Lentini, Syracuse, and Noto to Cape Passero, where he embarked for Malta. In this route he had an opportunity of examining most of the principal stratified formations in Sicily, and hopes to have clearly determined the exact place in the geological series to which many of them must be referred.

The formations described by the author are arranged under the eight following divisions:—

1. The oldest formation which he met with is a sandstone with a few subordinate beds of marl and limestone, occupying a great portion of the central chain of the island, and extending along part of the northern coast. Its exact age he was unable during his rapid tour to determine, but it is older than the Jura or Apennine limestone. In travelling along the coast eastward from Palermo, the author first came on this sandstone near the river Pilato, a few miles to the west of Cefalu; and the island eastward of this point principally consists of the same rock and its accompanying shales. In his route thence to Mistretta he passed over the great chain of the island, which in this part consists entirely of this sandstone, and attains a very great elevation. The mountain of Sancta Diana rises 3875 feet above the level of the sea, and is overtopped by many others within sight, on the loftiest of which, the Madonia, patches of snow were still visible on the 8th of June.

The dip of the sandstone strata is various; but they are in general highly inclined, and sometimes vertical. Their direction is for the most part parallel to the general direction of the chain itself,—namely, inclining from the north of east to the south of west. At Mistretta the strata are seen distinctly to dip away from an anticlinal line, which passes across the mountain of Sancta Diana, extends between the hill on which the castle stands, and the small hill of S. Catarina on its north, and thence across the valley to the east of Mistretta. At the Monte di Castelli, the highest point near Mistretta, the strata have two different bearings, one nearly east and west, and the other north and south; and a similar observation was made at Nicosia. The author directs attention to the fact, as indicative of the central chain having been raised during at least two distinct periods of elevation.

2. The formation next in order to the sandstone, and of more recent origin, is the limestone and dolomite composing the north-western part of the island, and which the author considers as the equivalent of the Jura or Apennine limestone. It rises in bold, precipitous cliffs flanking the bay of Palermo, and at the distance of about two miles inland bounds the rich plain which lies along the coast. The dolomite closely resembles that of the Tyrol, presenting a bold, rugged outline, without a trace of stratification, and having its naked sides traversed by numerous rents and fissures. Caves, sometimes containing bones, are frequent, formed probably by the enlargement of fissures by the action of water. The limestone, which frequently contains magnesia, is stratified, and the strata are often highly inclined.

3. The third formation distinguished by the author consists of marls and limestones containing Nummulites and Hippurites, and which he believes to belong to the chalk and green-sand formations of other parts of Europe. These beds are horizontal, and lie on trap-tufa and basalt. They were observed at the most southern extremity of Sicily: they extend from the village of Pachino to the sea—occupy the upper part of the island of Cape Passero—and form the base of the small island named the Isola delle Correnti.

4. The next rocks in the series are cretaceous limestones and marls of

of the older tertiary epoch. In the order of superposition they occupy a place immediately below the tertiary limestone next to be described, which contains shells of existing Mediterranean species, and is hence probably of much more recent origin.

5. The fifth formation is an extensive tertiary limestone, found both north and south of the great central range. Its prevailing character is that of a coarse, yellowish or white limestone, extensively quarried in several places as a building material. Most of its shells belong to species now existing in the Mediterranean, the most abundant being Pectens and Oysters. The genera *Cardium, Pectunculus, Arca,* with *Echini, Serpulæ,* and Corals, are also very common. In the plain of Palermo the strata are perfectly horizontal; but in the valley of the Oretus, where they lie close upon the dolomitic limestone, they are considerably inclined, and are higher by 100 feet than in the plain. A similar disturbance was observed at the Cape delle Mandre. At the south of the central chain the tertiary rocks are still more disturbed, being elevated to several thousand feet above the level of the sea. The direction of these inclined strata is parallel to that of the principal chain.

6. The next formation is a conglomerate still more recent than the upper tertiary beds last mentioned, and containing shells of species now existing in the Mediterranean. Its character varies in different situations, according to the nature of the rocks of which it is composed. It may be studied as well on the north coast as in the valleys to the south of the central chain, especially in that of the Limetus, between Palermo and Catania, and to the south of Syracuse. Its position, as well as fragments of tertiary rocks contained in it, prove it to be *posterior* to these; its sea shells attest its marine origin; and the perforations by Lithodomi prove it to have been covered by the waves prior to its elevation.

7. Of the same age with the preceding conglomerate is the Bone-breccia. Three bone-caves are enumerated by the author as situated in the immediate neighbourhood of Palermo. One of them, the Grotta de San Ciro, about two miles south-east of the town, is situated near the base of the magnesiferous limestone mountain of Grifoni, close upon the plain of Palermo; while the other two are in the mountain of Beliemi, about four miles to the west of the town, at a considerable elevation, being more than 300 feet above the level of the sea, and 100 feet higher than the cave at San Ciro.

The breccia at San Ciro is not confined to the cave itself, but forms a great part of the external talus, where it rests immediately on the upper tertiary beds, and has a thickness of about 20 feet. The breccia consists of numerous fragments of bones, with some rolled pieces and blocks of limestone cemented together by a little lime or clay; and it has some appearance of stratification, indicative of a deposit from water. The bones have been pronounced by Baron Cuvier to have been those of the Elephant, Hippopotamus, and Deer, with a few of a carnivorous animal of the genus Canis.

The author infers from a careful personal examination, that this breccia

breccia was deposited by water, and that subsequently to its formation and prior to its elevation, it remained long under the waves. This conclusion he believes to be justified by the appearance exhibited by the sides of the cave, which in some parts are smooth and polished as if long worn by water, and at others are perforated by Lithodomi. In this opinion he considers himself fully borne out by a bone-breccia lately discovered near the bay of Syracuse, about 70 feet above the level of the sea, and deposited in caves worn in the tertiary rocks. This breccia is of the same age as that at San Ciro, contains the bones of similar extinct quadrupeds, is intermixed with sea-shells, and has not only been worn by water since its formation, but its substance has been perforated by Lithodomi. From all these circumstances, considered in conjunction with the extent of the preceding newest tertiary deposits, the author considers it certain that the extinct quadrupeds, the bones of which are contained in the breccia, must have lived at a period long posterior to that in which the Mediterranean began to be inhabited by its present species of Mollusca, Radiata, and Zoophytes, and before the last convulsion which raised a great part of Sicily above the level of the sea.

The caves at Beliemi were not so minutely examined by the author as that at San Ciro. In one respect they possess much interest. They are situated at a greater height than the tertiary rocks have attained in that neighbourhood; and neither the caves themselves nor the bone-breccia have any appearance of marine action. The author thence infers that the breccia at Beliemi was above the surface of the sea at the time that the breccia at San Ciro was beneath it; and that their present heights mark the extent to which the tertiary formation at that part has been raised by the great convulsion, by which a large portion of Sicily has been elevated.

8. The last formation noticed by Dr. Christie is diluvium, of which he distinguishes two kinds differing in age. The older diluvium—answering, he conceives, to the *terrain de transport ancien* of Elie de Beaumont—consists of large rolled fragments of sandstone, with a few fragments of the tertiary rocks cemented by a sandy clay, is of the same age as the conglomerate and bone breccia, and occupies considerable heights on the sides and summits of the hills. The newer diluvium is quite distinct from the preceding, occupies only the bottom of the valleys, sometimes to great depth, and consists partly of rolled fragments of older rocks, even of the conglomerate, together with a great quantity of grey clay. They may both be distinctly seen in the valley of the Limetus.

In addition to the general conclusions already mentioned in the history of the bone-breccia, the author considers his observations as affording complete confirmation of the views of Elie de Beaumont regarding the epochs of elevation of the Sicilian mountains. The principal chain, extending across the island to the north of Castro Novo and Nicosia towards Messina, is not only sensibly parallel to the principal chain of the Alps, whence alone, according to Elie de Beaumont, the date of elevation must be the same; but the author contends

tends that both chains were elevated posterior to the formation of the conglomerate and older diluvium, and therefore that their periods of elevation are identical.

LINNÆAN SOCIETY.

Nov. 1 and 15.—A Paper was read on the Sexual Organs and mode of Impregnation in *Orchideæ* and *Asclepiadeæ*. By R. Brown, Esq., V.P.L.S., &c. &c.

Mr. Brown's principal object in this paper is to detail some observations on the structure and œconomy of the fecundating organs in these families, made by him chiefly in the present year. They have hitherto presented the most important objections to the prevailing theories of vegetable fecundation: but Mr. Brown thinks we are now as far advanced with respect to this function in these families, as in any other tribe of Phænogamous plants; and that upon the general problem of generation, additional light is more likely to be derived from them than any other part of the vegetable or animal kingdom.

Orchideæ.—Two opinions have been entertained as to the mode of impregnation in this family. Haller, Adanson, Curtis, C. K. Sprengel, J. K. Wachter, Schkuhr, Swartz, Salisbury, and Treviranus considered the direct application of the pollen to the stigma as essential to fecundation: while others, as Linnæus, Schmidel, Koelreuter, Stokes, Batsch, Richard, Du Petit Thouars, Link, Lindley, Bauer, and Mr. Brown himself, from the peculiarities observable in the structure and relative position of the sexual organs, have considered the direct contact of the pollen mass and stigma as improbable, and have consequently had recourse to other explanations of this function.

Wachter in 1799 was the first who succeeded in impregnating an orchideous plant by applying the pollen to the stigma,—a result which was confirmed in 1804 by Salisbury, and twenty years after by Treviranus.

These observers have sufficiently proved that impregnation is accomplished by the direct application of pollen to stigma: but no subsequent pheuomena resulting from the action of the stigma on the pollen is noticed.

Those authors who conceived that the pollen mass could not come into direct contact with the stigma, have attempted to explain the fecundation of *Orchideæ* in various ways.

Batsch in 1791 supposed that the only way in which the pollen could act on the ovarium in *Ophrydeæ* was by the retrogradation of the impregnating power through the caudicula to the gland beneath it; and this opinion was also that of Richard, who applied it to the whole order, and was entertained by Mr. Brown in 1810. It was also the opinion of Mr. Bauer, to whom it appears to have occurred as early as 1792.

The opinion of Du Petit Thouars was peculiar. He considered that the glutinous substance connecting the grains of pollen was the fecundating matter; that the elastic pedicel of the pollen mass, not formed before expansion, consisted of this gluten; and that in the expanded flower, the gluten which has escaped from the pollen is, in all

all cases, in communication with the stigma. The stigma he describes as a glutinous disk, from which a central cord of the same nature is continued through the style to the ovarium, where it divides into three branches, each of which divides into two, the six branches so formed running down, one on each side of the corresponding placenta to the base, giving off ramuli, which go to the ovula and separate them into groups. A communication is thus, in his opinion, established between the anthera and ovula, which he supposed were impregnated by an aura seminalis through their surface, and not, as he believes to take place in other families, through the funiculus.

In the spring of this year Mr. Brown renewed his observations on this family, and the results of his investigations are very curious and important. His attention was first directed to the relation which the lateral and generally rudimentary stamina bear to the other parts of the flower; and fully satisfied himself, that the opinion which he had expressed in his observations upon *Apostasia* was correct, viz. that they are placed opposite to the two lateral divisions of the inner series of the perianthium, and not, as had been before supposed, to the lateral divisions of its outer series. He then turned his attention to the composition of the stigma with respect to the relation which its lobes bear to the other parts of the flower and to the component parts of the ovarium. He satisfied himself that *Orchideæ* have in reality three stigmata more or less confluent in general, but in some cases distinct, and even furnished with styles of some length. These stigmata are opposite to the three outer divisions of the perianthium, and consequently terminate the axes of the component parts of the ovarium, which he regards as composed of three simple ovaria united by their ovuliferous margins,—a structure in which the ordinary relation of stigmata to placentæ is that here found.

In *Cypripedium* and *Apostasia*, in which the lateral stamina are perfect, and the middle one without anthera, all the lobes of the stigma are equally developed and of nearly similar form and texture, and, as Mr. Brown has proved by experiment in *Cypripedium*, are all equally capable of performing the function of the organ. But in most cases, that lobe which is opposite to the middle and perfect stamen and deriving its vessels from the same cord, does not perform the function of the organ, there being hardly an instance of a perfectly developed stamen and stigma placed opposite to each other, and having the same vascular supply. To this lobe the glands always belong, to which the pollen masses become attached, but from which they are always originally distinct. Its office, therefore, is essentially different from the lateral lobes, which are always present, more or less developed, and capable of performing their proper office. These lateral lobes are most developed in *Satyrinæ* or *Ophrydeæ*, especially in *Bonatea speciosa*, in which they have been mistaken for portions of the labellum. That they are, however, actually the efficient stigmata Mr. Brown has proved by experiment, in applying the pollen mass to their secreting surface, which was followed by the enlargement of the ovarium. In the ordinary structure, therefore, of *Orchideæ*, in which only one perfect stamen is produced, the corresponding

sponding stigma loses entirely or in part its function, which it regains in those cases where this stamen is destitute of anthera; and hence these organs, when perfect, are never placed opposite, but always alternate with each other.

The tissue of the perfect stigmata in *Orchideæ* is not materially different from that of other plants. It consists of densely approximated utriculi, which enlarge, and are subsequently separated from each other by a viscid secretion. The channel of the style has a similar structure, and undergoes similar changes previous to impregnation. In the unimpregnated ovarium, the upper portions which correspond to the axes of the placentæ, but which do not bear ovula, are neither secreting, nor do they consist of utriculi like those of the cavity of the style: and the same is observable in the six lines marginal to the three placentæ; and these lines, both above and at the margins of the placentæ, Mr. Brown calls the conducting surfaces of the ovarium.

The female organ is now in a proper state to be acted upon by the pollen; and Mr. Brown has satisfied himself that it acts by being brought into contact with the stigma, as Treviranus's experiments proved. He applied the pollen to the stigmata in several tribes of *Orchideæ*, and found that its grains, either in the entire mass or separately, soon produced tubes or *boyaux*, like those first described by Amici and Brongniart. One tube is emitted from a simple grain of pollen, their number consequently corresponding to that of the cells of the compound grain. These tubes acquire a great length, even while adhering to the grains which produce them, and have a diameter less than $\frac{1}{1000}$th part of an inch. They eventually separate from the grain while immersed in the viscid secretion of the stigma. They are cylindrical, neither branched nor jointed, with apparent interruptions within, probably from partial coagulations, on the walls, of the contained fluid. With a magnifying power of 150 Mr. Brown has not been able to observe granules in them even in their earliest state. With a power of 300 or 400, an extremely minute transparent granular matter may be detected.

The tubes thus produced from the pollen mass are generally very numerous, and form a cord, which passes through the channel of the stigma orstyle. On reaching the cavity of the ovarium, this cord divides into three parts, which are applied to those upper axes of the valves which are not placentiferous, and at the top of each placenta each of these three cords again divides into two branches, the six ultimate divisions thus produced passing down along the margins of the placentæ, along what has before been called the conducting surfaces of the ovarium. They descend to the base of the placentæ, with which they are nearly in contact; but Mr. Brown has not been able satisfactorily to trace branches from them mixing with the ovula. These cords are entirely composed of pollen tubes, and are undoubtedly essential to fecundation, but in what manner they operate is unknown. Mr. Brown adopts the opinion of M. Brongniart, that the *boyaux* are derived from the inner membrane of the grain, and believes the correctness of this opinion to be demonstrated in *Asclepiadeæ*, in which the membranes

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are entirely distinct. Their production he considers a vital action excited in the grain by the application of an external stimulus, which is afforded by the secretion on the surface of the stigma; and they derive nutriment either from the particles contained in the grain, or from the conducting surfaces with which they are in contact.

The first visible effect of the action of the pollen on the stigma is the enlargement of the ovarium, which, in those cases where it was reversed by torsion, untwists and resumes its original position.

After impregnation the ovulum enlarges, the nucleus disappears, probably from its acquiring greater transparency, and becoming confluent with the substance of the testa: soon after, a minute speck about the middle of the testa becomes visible, which is the commencement of the future embryo. At this period a thread may be traced from its apex nearly to the open end of the testa, consisting of a simple series of short cells, the lowermost one of which is probably the original state of what, from enlargement and deposition of granular matter, becomes the opake speck or rudiment of the embryo; the only appreciable changes in which are its increase in size and eventual cellular structure. In the ripe state it forms an ovate or spherical body, consisting of an uniform cellular tissue covered by a thin membrane, the base of which exhibits no indications of original attachment at that point, while at the apex the remains of the lower shrivelled joints of the thread are still often visible. The embryo, therefore, would be without albumen; the germinating point its apex, or that where the cellular thread is found; and the seed and funiculus are without vessels.

Asclepiadeæ.—The mode of impregnation in this family was supposed by Jussieu, Richard, Bauer, Treviranus, and by Mr. Brown, to be indirect; that is, that there was no immediate contact of the pollen with the stigma, but that the fecundating matter was conveyed from the mass through the arm and gland to the female organ.

At a very early period Gleichen had observed that the pollen masses were originally distinct from the glands,—a fact which Mr. Brown had afterwards stated in 1809, and which had equally been observed and delineated by Mr. Bauer. Gleichen also states, that before the masses unite with the glands they are removed from the cells of the anthera, and implanted into the wall of the tube which surrounds the ovaria, and that in this situation a white viscid substance hangs to them, which consists of tubes containing globules; and these tubes and their contents he considers as the early preparation for the formation of pollen. He remarks, that the tops of the styles are not originally connected with the pentagonal body, and therefore, that impregnation does not usually take place until the true stigmata, or those extremities of the styles on which vesicles and threads are observable, have penetrated through the substance of the pentagonal body, and are on a level with its apex. At the same time, he is disposed to believe that insects may occasionally assist in the function by carrying the fecundating matter directly to the stigmata, even before they enter the pentagonal body.

Sprengel in 1793 asserts that insects extract the pollen masses from

from the cells, and apply them to the apex of the stigma, which, being a secreting part, is calculated to act on the oily matter exuding from the surface of the pollen mass.

In 1829 Ehrenberg describes the grains of pollen as contained in the proper membrane of the mass, which bursts in a regular manner, and as having each a tube, which tubes collectively are directed towards the point of dehiscence. He does not say how they communicate with the stigma, and supposes that they form an integrant part of the grain, without being produced by the action of an external stimulus.

In July last Mr. Brown resumed his investigations into the structure and functions of the organs of this family. He verifies observations made by Mr. Bauer so early as 1805, that the masses are cellular, each cell containing a single grain. These cells he considers as the outer membranes of the grains of which the inner membranes are the grains described by Treviranus without tubes, and by Ehrenberg after their production.

He found that the agency of insects was necessary to effect fecundation; that the pollen masses were actually removed from the cell of the anthera, and immersed in the fissures formed by the projecting alæ of the antheræ, the descending process of the stigma near its flexure being broken, so that the mass was entirely separated from the gland. The pollen mass was so placed in the fissure, that its inner or convex edge was closely pressed to that point where the tube of the united filaments is joined to the base of the corresponding angle of the stigma. On separating them, a white cord, consisting of slender tubes, was observed issuing from the gibbous part of the edge which had burst. On opening the mass, these tubes were found to proceed each from a grain of pollen, all directed towards the aperture. They were like those described in *Orchideæ*. The cord had opened a passage for itself through the membrane, or, rather, had separated the upper edge of this membrane from the base of the stigma, to which it was before united. It then passed along the surface of the base of the stigma until it arrived at its articulation with the two styles; then inclined towards the inner side of the apex, which is in some degree exposed. On opening the cavity of the style, the cord was seen passing down the centre to the commencement of the placenta, where it appears to terminate.

These appearances, which were the result of the application of the pollen to the base of the stigma by insects, were equally obtained by Mr. Brown's removal and application of the mass. He found that the convex edge of the mass must be the part applied, though he could detect nothing peculiar in its structure or appearance. At present he has not had sufficient opportunities to discover how impregnation is effected in those plants of this family which have erect pollen masses; for though he has succeeded in producing the tubes in *Hoya carnosa*, he could not get them to communicate with the stigma, nor can he conjecture how this is to be effected.

ASTRONOMICAL SOCIETY.

June 10.—The following communications were read:—

I. Observed occultation of Jupiter and his satellites by the moon, June 1, 1831.

1. By the Astronomer Royal, giving the contact, immersion of Jupiter's centre, and total immersion, by four observers; and the emersions of Jupiter and the four satellites, by seven observers.

2. By Mr. Snow, containing the complete observations of all the immersions and emersions, made in Savile Row, together with the observed transits by which the clock error was determined.

Mr. Snow observes:

"Between the contact of the moon's limb with the planet, and the planet's disappearance, 1′ 32″ elapsed; and during that time no change of light, colour, or motion, took place in the planet, which remained uniformly of a rather more dusky colour than the moon. Both the planet and moon were in a violent state of undulation during the immersion; but the moon's undulation was seen quite distinctly upon the planet's disc, and differed, for that small arc, in no respect from the undulation of the rest of the limb. I watched very particularly for any projection of the planet upon the moon's edge, but could see none.

"It is perhaps worth mentioning, that the planet's second limb, during, and for several seconds after its final emersion, and in both states of the adjustment of the telescope, appeared to have a very considerable curvature towards the moon's dark limb. The planet, however, soon returned to a shape nearly spheroidal."

3. By Captain Smyth, containing the same observations. Captain Smyth also remarks:

"Although the emersions of the satellites were perfectly distinct, they were certainly not so instantaneous as those of the small stars; which I think was more owing to light than disc. Jupiter entered into contact rather sluggishly; but though the lunar limb was tremulous from haze, there was not the slightest loss of light. Faint scintillating rays preceded the emersion, which was so gradual, that, as the planet re-appeared, the edge of the moon covered it with a perfectly *even* and black segment, which cut the belts distinctly, and formed clear sharp cusps, slowly altering until the whole body was clear. There was no appearance of raggedness from lunar mountains, and Jupiter's belts were superbly plain while emerging; but there was not the slightest distortion of figure, diminution of light, or change of colour."

In allusion to the deviation seen by Messrs. Ross and Comfield, which is inserted in the Memoirs, Captain Smyth mentions the following fact:

"On Thursday the 26th of June, 1828, the moon being nearly full, and the evening extremely fine, I was watching the second satellite of Jupiter as it gradually approached to transit its disc. My instrument was an excellent refractor, of $3\frac{3}{4}$ inches aperture, and

and 5 feet focal length, with a power of 100. It appeared in contact at about half-past 10, by inference, and for some minutes remained on the edge of the limb, presenting an appearance not unlike that of the lunar mountains coming into view during the first quarter of the moon, until it finally disappeared on the body of the planet. At least twelve or thirteen minutes must have elapsed, when, accidentally turning to Jupiter again, to my astonishment I perceived the same satellite *outside the disc!* It was in the same position as to being in a line with the apparent lower belt, where it remained distinctly visible for at least four minutes, and then suddenly vanished."

The same phænomenon was also observed on the same evening, at different places, by Mr. Maclear and Dr. Pearson.

II. A letter from Mr. Dawes to Mr. Herschel, giving observations of the double stars, 70 Ophiuchi, ξ Ursæ Majoris, 44 Bootis, γ Virginis, η and σ Coronæ, Castor, γ Leonis, and ε Bootis, made during the last and present year. Mr. Dawes is of opinion that the apparent discs are more affected by the aperture of the telescope than by any other circumstance; and that this is the principal reason why a reflector presents a smaller image of a fixed star than an achromatic of equal illuminating power.

III. The reading of Mr. Herschel's paper on the measures of 364 double stars was completed.

This paper contains the micrometrical measures of the angles of position and distances of 364 double stars, observed by Mr. Herschel with the seven-feet equatorial in his possession at Slough, the same which was used by Sir James South in his observations at Passy, and in a part of the measures of double stars in the *Philosophical Transactions* for 1824. The individual measures, being too numerous, are not stated; but the mean results of each night's observation are set down in tabular order, with a weight attributed to each, indicative of the degree of confidence which the observer himself attributes to them, and with notes attached, descriptive of any peculiar circumstances in the observation proper to be recorded. They comprise the results of 735 sets of measures, or from 6000 to 8000 individual observations, obtained under all atmospheric circumstances, in the years 1828, 1829, and 1830. Almost all the stars observed are taken from the great catalogue of Struve.

The author prefaces his observations by a comparison of his results with those already obtained by other observers in the cases where his stars *have* been observed by others, and draws a conclusion not unfavourable to the general accuracy of his angles of position; although, in some individual cases, considerable discrepancies and even grievous errors are admitted. In the course of this comparison he is led to point out some stars as having so materially changed, at least apparently, that he is induced to recommend them for further observation, as being possibly of a binary nature. With regard to his distances, he professes himself much dissatisfied, owing to an imperfection in the micrometer attached to the instrument,

 which

which is the same with that used in all its former measurements, and of which the cause has been detected too late to remedy the evil.

After noticing some peculiarities in his mode of observation, and in particular its use of a red illumination of the wires, which he regards as a great improvement, he enters into a more particular examination of the bearing of his present results on the theory and history of the following stars, whose motions he considers as fully demonstrated by them, although some of them had previously been brought into question.

η Cassiopeæ.	γ Virginis.	39 Draconis.
η Persei.	η Coronæ.	ε^1 Lyræ.
Castor.	μ^2 Bootis.	ε^2 Lyræ.
ζ Cancri.	49 Serpentis.	ζ Sagittæ.
ω Leonis.	σ Coronæ.	61 Cygni.
γ Leonis.	μ Draconis.	δ Equulei.
ξ Ursæ.	70 Ophiuchi.	ζ Aquarii.

Among these the most remarkable of his conclusions refer to Castor, ζ Cancri, ξ Ursæ, γ Virginis, and η Coronæ. In the cases of Castor and γ Virginis, he is enabled, by the kindness of Professor Rigaud, to produce observations of their positions by Bradley and Pound, which are peculiarly valuable, as they carry back the history of these stars upwards of a century, and enable us to form a much better judgement than heretofore of their orbits, both which appear to be ellipses of considerable elongation. He describes a very ready and easy graphical process by which these orbits may be approximately laid down, and exemplifies it on γ Virginis, respecting which he concludes that the present approach of the stars, and increase of their angular velocity, will still go on for several years, until their distance is reduced to less than a single second, which, considering the brightness of both individuals, will render this a single star to all but the very finest telescopes. Castor he also considers likely, ere very many years have elapsed, to become a close double star, and again to open to a much more considerable distance than at present.

In the case of ζ Cancri, he is led by his observations to conclude that the motion is retrograde instead of direct, and much more rapid, so that this star has nearly completed a whole revolution. This conclusion is fully confirmed by his observations of the present spring (1831), and also by the observations of Mr. Dawes, which have been communicated to him very recently, and which agree in a very remarkable manner with his own, and actually suffice to trace the bimestral motion of the star, as had been previously done in the case of ξ Ursæ. η Coronæ, too, offers, if all the observations can be trusted, a still more remarkable instance of quick rotation, being already considerably advanced in its *second* revolution since Sir Wm. Herschel's first discovery of it; but this will require further confirmation, owing to the extreme difficulty of the measures. The

The periods of these two stars may be stated respectively at 55 and 43 years; that of ξ Ursæ at $56\frac{1}{2}$, which last determination agrees nearly with that of M. Savary, who has assigned $58\frac{1}{2}$ years for this element. The period of 70 Ophiuchi has been determined by Professor Encke at 73·862 years.

Lastly, Mr. Herschel subjoins some practical remarks on the management and action of telescopes, and takes occasion to introduce a set of measures of the positions of Saturn's satellites, taken under remarkably favourable circumstances, and also a single observation of the place of the comet of 1830, the only one he could succeed in procuring.

In a subsequent letter received from Mr. Herschel, he alludes to a new method of taking the angles of position, viz., by reflected as well as by direct vision. He conceives that this method possesses two capital advantages: 1st, That the quantity directly measured is double the quantity sought, by which, of course, the error of determination is halved; and, 2ndly, That the error of the zero is destroyed altogether, the double arc being given by the difference of readings in the limb of one and the same circle, maintained in one and the same position. Mr. Herschel also adds, that subsequent and very satisfactory measures of η Coronæ fully confirm the conclusion above mentioned with regard to the revolution of that star.

IV. On the dependence of a clock's rate on the height of the barometer, by the Rev. Dr. Robinson.

The rate of a clock of good workmanship may be assumed to depend on three things; first, the rate at a given temperature and barometric pressure: secondly, on the variation of temperature, as shown by the thermometer; and thirdly, upon the atmospheric pressure, measured by the barometer. The variation of temperature not only affects the materials of the pendulum, but, along with the atmospheric pressure, greatly modifies the action of the air in the way of *buoyancy* and *inertia*, &c. Possibly, the irregular action of the wheelwork, the diminution of arc from the thickening of the oil on the pallets, and even the hygrometric state of the air, may sensibly alter the rate of a clock; but the present memoir is confined to the consideration of the first-mentioned sources of error.

As the changes are minute, Dr. Robinson formed equations of condition, where the quantities sought were the rate of the clock at 49° of the thermomer, and 29·5 inches of the barometer, and the retardations corresponding to a rise of 1° and 1 inch in each of these instruments respectively. The absolute gain or loss of the clock was determined with the utmost care.

"A thermometer placed in the clock-case, with its bulb three inches before the pendulum rod, and on a level with the cover of the jar, and a barometer with its cistern at the same level, were noted morning and evening at half-past nine; a time chosen, not merely as likely to give mean results for the whole day, but also because it nearly bisects the average period of evening observations. The thermometer was read through a square of plate glass, cemented

in

in the door of the case; and this being screwed close, (as described in the *Armagh Observations* for 1828,) all free communication with the air is precluded, and the changes of temperature within it are slower and more regular than would be possible in clocks of the ordinary construction. This made observations at other than the stated periods unnecessary. The barometer was observed whenever its movements seemed irregular, and the mean for the day corrected, if necessary, by interpolation."

Dr. Robinson then shows that the equations, when treated by the method of least squares, give, for the gaining rate of the clock at the standard temperature and pressure, $0^s,263$; for the gain by an increase of 1° in the temperature (his clock was over compensated), $0^s,069$; and for the gain by a depression of 1 inch in the barometer, $0^s,241$. As the mere effect of the air's buoyancy on the mercurial pendulum is $0^s,15$ for a change of 1 inch in the barometer, the remainder is due to the inertia of the air carried along with the pendulum in its oscillations.

Dr. Robinson finds that the barometrical pressure has no sensible effect upon the *arc of vibration*, and, consequently, does not affect the clock through this cause.

With respect to the barometric variation of rate, having once determined its co-efficient, nothing is easier than to allow for it when any sudden change occurs: it may, however, be corrected mechanically with the utmost facility. A rise in the barometer tends to make a clock go slower. Suppose a syphon barometer to be attached to the pendulum, then a rise in the barometer will transfer a portion of the mercury in the syphon to the tube, and the effect will be the same as if a small weight were shifted from the syphon end to the tube end of the barometer. Now if the syphon be so adjusted to the pendulum, as to bore and situation, that this alteration of the mercury in the barometer will accelerate the rate as much as the increase of the air's buoyancy and inertia retard it, the clock will be unaffected by any variation of the atmospheric pressure.

Dr. Robinson shows how the syphon barometers (he places two, one on each side of the pendulum rod) may be constructed and adjusted for any particular case.

"I had previously recognised the influence of the barometer on the clock before the mercurial pendulum was applied; and, without employing the method of minimum squares, satisfied myself that the effect was even greater, being nearly 0·3 for an inch. In examining this point for any given clock, it is however to be remembered, that it is always necessary to make the probable errors of observation considerably less than the quantity sought; otherwise, no conclusive result can be obtained. This may be done, either by taking the rate for intervals of several days, or by observing several stars on each night."

A List

A List of Stars observed with the Moon, in the present year, at the Royal Observatory, Greenwich.

1831.		h m s	1831.		h m s
Jan. 6.	Spica Virg.	13 17 6,08	Feb. 26.	48 Leonis	10 26 25,46
	☽ 2 L	14 2 43,42		o^1 Sextantis	10 37 44,26
				o^2 ——	10 38 58,17
7.	Spica Virg.	13 17 6,94		☽ 2 L	10 54 24,16
	☽ 2 L	14 50 2,08			
			Mar. 23.	☽ 1 L	8 45 15,92
24.	111 Tauri	5 15 34,20		π^2 Cancri	9 6 40,57
	☽ 1 L	5 24 33,76			
	N Tauri	5 38 35,46	27.	(50) Leonis	11 15 29,88
	χ^4 ——	5 54 27,40		(77) ——	11 20 6,86
				υ ——	11 29 8,96
25.	☽ 1 L	6 27 32,76		☽ 1 L	12 13 22,98
	G Geminor.	6 41 7,24			
	(281) ——	6 48 53,34	April 20.	☽ 1 L	9 24 42,42
	ξ ——	6 55 6,04		ψ Leonis	9 35 40,74
26.	☽ 1 L	7 30 6,74	May 22.	☽ 1 L	13 18 27,40
	g Geminor.	7 37 22,62		b^3 Virginis	13 28 2,80
				(174) ——	13 36 25,84
Feb. 5.	1 β Scorpii	15 56 46,70			
	☽ 2 L	15 8 6,62	24.	λ Virginis	14 11 17,78
	Antares	16 20 12,40		(127) Libræ	14 29 20,18
				μ ——	14 41 23,18
19.	☽ 1 L	4 4 11,04		☽ 1 L	14 53 6,26
	h^3 Tauri	4 13 51,36			
	π ——	4 18 22,86	25.	ξ^3 Libræ	15 22 29,04
	ϱ ——	4 25 34,94		γ ——	15 27 24,72
				☽ 1 L	15 41 37,50

[The following information respecting this list is given in the Monthly Notices of the Society, Vol. II. p. 42. "N.B. The Astronomer Royal having kindly offered to furnish the Society, from time to time, with the observations of moon-culminating stars made at Greenwich, the first series is here given for the present year; and they will be continued in the subsequent Monthly Notices, as they are received; by which means they will become more speedily accessible to those persons who are desirous of making comparisons of this kind."]

ZOOLOGICAL SOCIETY.

August 9, 1831. Dr. Horsfield in the Chair.

A letter from George Swinton, Esq., of Calcutta, Corr. Memb. Z. S., addressed to the Secretary, was read, announcing the transmission to England, as a present to the Society, of an entire *Dugong*, preserved in spirit and brine, which he hoped would arrive in a sufficiently perfect state to admit of its dissection.

Specimens of two species of *Bats*, presented to the Society with a numerous and valuable collection of birds formed at Madras by Josiah Marshall Heath, Esq., F.L. & Z.S., were exhibited, and Dr. Horsfield identified them as the *Megaderma Lyra*, Geoff., and a new species

species of the genus *Nycticejus*, Rafin. He pointed out in the former some discrepancies in the colouring from that described by M. Geoffroy Saint-Hilaire, apparently from a specimen preserved in spirit; the individual before the Meeting agreeing much more nearly with the colours as recently described by M. Isidore Geoffroy Saint-Hilaire, from whose description it scarcely differed, except in the less intensity of the rufous tinge of the tips of the hairs of the upper surface.

Of the *Nycticejus* two specimens were exhibited, on which Dr. Horsfield pointed out the characters by which that group had been generically distinguished from *Vespertilio* as circumscribed by modern authors. He remarked on the geographical distribution of the genus, which might be regarded altogether as an American form, were it not for the existence of a species in Java described by him in his 'Zoological Researches' as the *Vespertilio Temminckii*, and of the present species obtained on the Continent of India. As the second Indian species of this group, he regarded the present acquisition as peculiarly interesting. It is considerably larger than the Javanese species, from which it differs also remarkably in its colouring.

Dr. Horsfield thus characterized and described the species:

NYCTICEJUS HEATHII. *Nyct. capite cuneato supra lateribusque planis, auriculis capite brevioribus oblongis rotundatis margine exteriore parum excisis trago elongato falcato, vellere pilis sericatis brevissimis, notæo fusco, gastræo fulvo.*

Long. corporis (caudâ inclusâ), 6 unc.: expansio extremitatum anteriorum, 18 unc.

The head is of moderate length, nearly even above and compressed at the sides. The muzzle is broad and abruptly terminated. The nose is slightly emarginate. The eyes ———. The mouth is proportionally small. The lips are not rugose, and are nearly covered with delicate hairs. The ears are shorter than the head; the auricle oblong, erect, rounded, naked and slightly indented posteriorly, terminating below in a small lobule; the *tragus* linear, erect, falciform, and shorter than the auricle.

The animal is uniformly and thickly covered by a short, very soft, delicate silky hair, closely applied to the skin: this hair is about a line in length on the back, but shorter and more delicate on the head; on the breast it is somewhat longer and downy. The colour of the body and hair above is brown with a tawny hue; underneath fulvous with a slight tendency to gray; the tint being uniformly distributed over the throat, breast, *abdomen* and sides. The transparent membrane is uniformly brown.

The collection of *Birds* formed by Major James Franklin, F.R.S. &c., on the banks of the Ganges and in the mountain chain of Upper Hindoostan, and presented to the Society by the Physical Committee of the Asiatic Society of Calcutta, (which had been laid on the table on the 23rd November last,) was again exhibited. The exhibition had been commenced at the previous Meeting of the Committee, when the *Raptorial* and *Insessorial Birds* were brought under the notice of the Members present; and it was now concluded by the *Rasorial*,

Rasorial, Wading, and *Swimming Birds*. On the former occasion, Mr. Vigors, and on the latter, Mr. Yarrell, availed themselves of the opportunity to remark on the geographical distribution of many of the species contained in the collection, and on other points connected with their history. They were exhibited in the order of the following

Catalogue of Birds (systematically arranged) which were collected on the Ganges between Calcutta and Benares, and in the Vindhyian hills between the latter place and Gurrah Mundela, on the Nerbudda, by Major James Franklin, F.R.S. &c.

ORDO I. RAPTORES.

Fam. FALCONIDÆ.

Sub-Fam. *Aquilina.*—Genus *Aquila.*

1. AQUILA VINDHIANA. *Aq. pallidè brunneo variegata; capite, pectore, remigibus secundariis, caudâque saturatioribus, hujus apice albido graciliter marginato; remigibus primariis nigris; capitis collique plumis pallido-rufo lanceolatis.*
   Longitudo 26 unc.
   Cawnpoor Eagle, Lath. ?

Sub-Fam. *Falconina.*—Genus *Falco.*

2. *Falco Subbuteo*, Linn. *Hobby*, Penn. *Le Hobereau*, Buff.
3. *Falco Chicquera*, Daud. *Chicquera Falcon*, Lath. *Le Chicquera*, Le Vaill.
4. *Falco Tinnunculus*, Linn. *Kestril*, Penn. *La Cresserelle*, Buff.

Sub-Fam. *Buteonina.*—Genus *Buteo.*

5. *Buteo Bacha.* *Falco Bacha*, Daud. *Bacha Falcon*, Lath. *Le Bacha*, Le Vaill.

Genus *Circus.*

6. CIRCUS TEESA. *Circ. capite corporeque rufo-brunneis, plumarum rhachibus fuscis; dorso imo, rectricibusque ferrugineis, his fasciis subobsoletis fuscis septem circiter notatis; remigum tectricibus abdomineque albescenti notatis; femorum tectricibus crissoque rufescenti-albis; fronte, gulâ, nuchæque fasciâ gracili albis; rostro pedibusque flavis, illius apice nigro.*
   Longitudo 17½.
   Zuggun Falcon, Lath. ?
7. *Circus cyaneus.* *Falco cyaneus*, Linn. *Hen Harrier*, Penn.
8. *Circus melanoleucus.* *Falco melanoleucus*, Gmel. *Black and white Indian Falcon*, Penn. *Le Tchoug*, Le Vaill.
9. *Circus rufus*, Briss. *Moor Buzzard*, Penn. *Le Busard*, Buff.

Sub-Fam. *Milvina.*—Genus *Elanus*, Savigny.

10. *Elanus Melanopterus*, Leach. *Le Blac*, Le Vaill.

Fam. *Strigidæ.*—Genus *Otus.*

11. OTUS BENGALENSIS. *Ot. pallidè rufescens, fusco alboque undulatim variegatus; nuchæ pectorisque plumis in medio strigâ latâ brunneo-nigrâ notatis; abdomine fusco graciliter fasciato; remigibus rectricibusque lateralibus prope apicem brunneo fasciatis, his mediis per totam longitudinem similiter notatis.*

 Longitudo

Longitudo 20.

Dr. Latham alludes to this as a variety of the *great-eared Owl.*

Genus *Noctua.*

12. Noctua Indica. *Noct. cinereo-brunnea; capite guttis parvis albis, alis grandioribus notatis; abdomine albo, maculis brunneis lunulatis notato; remigibus rectricibusque albo fasciatis; regione circumoculari, gulâ, fasciâque subgulari ad aures extendente albis.*
   Fœm. *magis rufescens, abdomine magis fasciatim maculato.*
   Longitudo 9.
   Indian Spotted Owl, Lath.?

Ordo II. INSESSORES.

Tribus Fissirostres.

Fam. *Meropidæ.*—Genus *Merops.*

13. *Merops Philippinus,* Linn. *Philippine Bee-eater,* Lath. *Grand Guêpier des Philippines,* Buff.
14. *Merops viridis,* Linn. *Indian Bee-eater,* Lath. *Guêpier à collier de Madagascar,* Buff.

Fam. *Hirundinidæ.*—Genus *Hirundo.*

15. *Hirundo Klecho,* Horsf. *Klecho Swallow,* Lath. *Hirondelle longipenne,* Temm.
16. Hirundo filicaudata. *Hir. supra purpurascenti-atra, remigibus fuscis; corpore subtùs maculisque rectricum omnium lateralium albis; capitis vertice rufo; rectrice utrinque laterali elongato, ad apicem gracillimo.*
   Statura *Hir. ripariæ.*
   Wire-tailed Swallow, Lath.
17. *Hirundo riparia,* Linn. *Sand Martin,* Penn. *L'Hirondelle de rivage,* Buff.

Genus *Cypselus.*

18. *Cypselus affinis,* Hardw. *Allied Swift,* Hardw.
19. *Cypselus Palmarum,* Hardw. *Balassian Swift,* Lath.

Fam. *Caprimulgidæ.*—Genus *Caprimulgus.*

20. Caprimulgus monticolus. *Cap. pallidè cinereo-brunneo, rufo, fuscoque sparsim variegatus; abdomine rufescenti-fusco fasciato; remigibus secundariis rufo nigroque fasciatis, primariis brunnescenti-nigris, quatuor externis fasciâ latâ albâ in medio notatis; rectricibus sex mediis fasciis gracilibus nigris undulatis, duabus utrinque lateralibus albis apicibus brunneis.*
   Fœm. *fasciâ alarum rufâ; caudâ concolori (sine albo).*
   Longitudo 10.
   Great Bombay Goatsucker, Lath.?
21. *Caprimulgus Asiaticus,* Lath., Ind. Orn. *Bombay Goatsucker,* Lath.

Fam. *Halcyonidæ.*—Genus *Alcedo.*

22. *Alcedo Bengalensis,* Gmel. *Little Indian Kingsfisher,* Edw.
23. *Alcedo rudis,* Linn. *Black and white Kingsfisher,* Edw.

Genus *Halcyon.*

24. *Halcyon Smyrnensis. Alcedo Smyrnensis,* Linn. *Smyrna Kingsfisher,* Lath. *Martin pêcheur de la côte de Malabar,* Buff.

Tribus

Tribus Dentirostres.

Fam. *Muscicapidæ.*—Genus *Muscicapa.*

25. *Muscicapa Banyumas,* Horsf. *Banyumas Flycatcher,* Lath. *Gobe-mouche Chanteur,* Temm.

26. *Muscicapa nitida,* Lath., Ind. Orn. *Nitid Flycatcher,* Lath.

Genus *Muscipeta.*

27. *Muscipeta Paradisi. Muscicapa Paradisi,* Linn. *Paradise Flycatcher,* Lath. *Gobe-mouche Tchitrec-be, roux et blanc,* LeVaill.

28. *Muscipeta peregrina. Parus peregrinus,* Gmel. *Crimson-rumped Flycatcher,* Lath. *Gobe-mouche Oranor,* Le Vaill.

Genus *Rhipidura.*

29. Rhipidura albofrontata. *Rhip. capite colloque nigris; dorso cinereo-nigro; alis caudâque fusco-nigris; fasciâ subgracili frontali super oculos ad nucham extendente, pectore, abdomine, maculis tectricum alarum, apicibusque rectricum, duabus mediis exceptis, albis.*
Longitudo 6.
White-browed Flycatcher, Lath.?

30. Rhipidura fuscoventris. *Rhip. capite nigro; dorso abdomineque cinereo-nigris; alis caudâque fusco-nigris; strigâ brevi superciliari colloque in fronte albis; rectricum trium lateralium apicibus albescentibus.*
Longitudo $7\frac{1}{2}$.
Broad-tailed Flycatcher, Lath.?

Fam. *Laniadæ.*—Genus *Ocypterus.*

31. *Ocypterus leucorhynchus. Lanius leucorhynchus,* Linn. *White-billed Shrike,* Lath. *Pie-grièche de Manille,* Buff.

Genus *Edolius.*

32. *Edolius cærulescens. Lanius cærulescens,* Linn. *Fork-tailed Indian Butcher-bird,* Edw.

Genus *Lanius.*

33. Lanius muscicapoïdes. *Lan. brunnescenti-cinereus subtus albescens; strigâ superciliari rufescenti-albâ; alis rectricibusque fusco-brunneis, his duabus lateralibus albis basi notâque ad apicem fusco-brunneis.*
Fœm. aut Mas jun. *capite corporeque suprà albido maculatis.*
Longitudo $6\frac{1}{2}$.
Keroula Shrike, Lath.?

Genus *Collurio.*

34. *Collurio Excubitor. Lanius Excubitor,* var. Linn. *Cinereous Shrike,* var. C. Lath.

35. *Collurio erythronotus,* Proceed. Zool. Soc. p. 42. *Grey-backed Shrike,* Lath.?

36. Collurio nigriceps. *Col. capite suprà, nuchâ, alis, caudâque nigris; gulâ, pectore, abdomine medio, maculâque in medio alarum, albis; dorso cinereo; scapularibus, uropygio, abdominis lateribus, crissoque rufis.*
Longitudo $8\frac{1}{2}$.
Indian Shrike, Lath.?

37. *Collurio Hardwickii,* Proceed. Zool. Soc. p. 42. *Bay-backed Shrike,* Lath.?

Genus *Graucalus.*

38. *Graucalus Papuensis,* Cuv. *Corvus Papuensis,* Gmel. *Papuan Crow,* Lath.

Genus *Ceblepyris.*

39. *Ceblepyris cana,* Temm. *Muscicapa cana,* Gmel. *Ash-coloured Flycatcher,* Lath.
40. *Ceblepyris fimbriatus,* Temm. *Echenilleur frangé,* Temm.

Fam. *Merulidæ.*—Genus *Pitta.*

41. *Pitta brachyura.* *Corvus brachyurus,* Linn. *Short-tailed Crow,* var. B. Lath. *Short-tailed Pie,* Edw.

Genus *Oriolus.*

42. *Oriolus Galbula,* Linn. *Golden Oriole,* Lath. *Le Loriot,* Buff.
43. *Oriolus melanocephalus,* Linn. *Black-headed Oriole,* Lath. *Loriot de la Chine,* Buff.
44. Oriolus Maderaspatanus. *Or. fronte, corpore suprà, tectricibus alarum, abdomineque luteis; capite suprà, genis, remigibus, notâque medianâ rectricum fusco-atris; gulâ albâ striis fusco-atris.*
Longitudo 9.
Oriolus Galbula, var. γ. Lath. *Yellow Indian Starling,* Edw. *Yellow Starling from Bengal,* Albin.

Genus *Turdus.*

45. *Turdus macrourus,* Gmel. *Long-tailed Thrush,* Lath.
46. *Turdus Saularis.* *Gracula Saularis,* Linn. *Pastor Saularis,* Temm. *Little Indian Pie,* Edw.

Genus *Timalia.*

47. Timalia Chataræa. *Tim. suprà pallidè brunnescenti-, subtùs rufescenti-cinerea; capite corporeque suprà lineis fuscis striatis; rectricibus fusco obsoletè fasciatis; rostro pallido.*
Longitudo 9¾.
Gogoye Thrush, Lath.?
48. *Timalia pileata,* Horsf. *Pileated Thrush,* Lath.
49. Timalia hypoleuca. *Tim. suprà rufescenti-brunnea, subtùs alba; alis rufis; his caudâque subtùs cinereis, rectricibus fusco obsoletè fasciatis; rostro nigro.*
Longitudo 6½.
50. Timalia hyperythra. *Tim. suprà olivascenti-brunnea; capite in fronte corporeque toto subtùs rufis; caudâ supernè fusco obsoletè fasciatâ; rostro pallido.*
Longitudo 5.

Genus *Ixos.*

51. *Ixos jocosus.* *Lanius jocosus,* Linn. *Jocose Shrike,* Lath.
52. *Ixos Cafer.* *Turdus Cafer,* Linn. *Cape Thrush,* Lath. *Le Courouge,* Le Vaill.
53. *Ixos fulicata.* *Motacilla fulicata,* Linn. *Sooty Warbler,* var. Lath. *Traquet noir des Philippines,* Buff.

Fam. *Sylviadæ.*—Genus *Iora.*

54. *Iora scapularis,* Horsf. *Scapular Wagtail,* Lath.

Genus

Genus *Sylvia.*

55. *Sylvia Hippolais,* Lath. Ind. Orn. *Lesser Pettichaps,* Lath. *Reed Wren,* Lath.

This is the bird alluded to under Dr. Latham's *Reed Wren,* as an Indian variety called *Tickra* and *Ticktickee.*

Genus *Prinia.*

56. Prinia cursitans. *Prin. corpore suprà pallidè brunneo, fusco striato; gulâ juguloque albis; abdomine rufescente; rectricibus mediis fuscis, omnibus subtùs ad apicem fasciâ nigrâ albo terminatâ notatis.*
Longitudo 4.

57. Prinia macroura. *Prin. suprà grisescenti-brunnea; capite, alis, uropygioque subrufescenti tinctis; subtùs ferrugineo-albida; rectricibus quatuor mediis saturatioribus fusco obsoletè fasciatis, subtùs ad apicem fusco leviter notatis.*
Longitudo $5\frac{1}{2}$.

58. Prinia gracilis. *Prin. cinereo-grisea; dorso, alis, caudâque olivascentibus; gulâ, pectore, abdomineque subtùs albidis; rectricibus subtùs griseis fasciâ nigrâ albo terminatâ notatis.*
Longitudo $4\frac{7}{10}$.
Foodkey Warbler, Lath. ?

Genus *Motacilla.*

59. Motacilla picata. *Mot. capite, collo, corporeque suprà nigris; strigâ utrinque superciliari alterâque longitudinali alarum, corpore subtùs, rectricibusque duabus lateralibus albis.*
Longitudo 9.
Pied Wagtail, Lath. pl. 104.

60. *Motacilla flava,* Linn. *Bergeronnette jaune,* Buff., & *Bergeronnette de printemps,* Buff. *Yellow Wagtail,* Lath.

This is the Indian bird alluded to by Dr. Latham under the head of *Yellow Wagtail,* called *Peeluck,* which is its Indian name.

Genus *Saxicola.*

61. *Saxicola rubicola,* Temm. *Stone Chat Warbler,* Lath.

Genus *Phœnicura.*

62. *Phœnicura atrata,* Jard. & Selb. *Indian Redstart,* Iid.

Fam. *Pipridæ.*—Genus *Parus.*

63. *Parus atriceps,* Horsf. *Mésange cap-négre,* Temm.

Tribus Conirostres.

Fam. *Fringillidæ.*—Genus *Alauda.*

64. Alauda Chendoola. *Al. suprà pallidè grisescenti-brunnea, plumis fusco in medio notatis; corpore subtùs strigâque superciliari albis; rectricibus brunneis, duarum utrinque lateralium pogoniis externis albis; pectore brunneo maculato, capite cristato.*
Statura *Al. arvensis,* Linn.

65. Alauda Gulgula. *Al. pallidè rufescenti-brunnea, plumis in medio latè et intensè brunneo lineatis; subtùs albescens, pectore brunneo lineato; femoribus rufescentibus; rectricibus brunneis, externâ utrinque ferè totâ, secundæ pogonio externo, albis.*
Statura ferè præcedentis.

Genus

Genus *Mirafra.*

66. *Mirafra Javanica*, Horsf. *Alouette mirafre*, Temm.

67. MIRAFRA PHŒNICURA. *Mir. pallidè cinereo-brunnea; corpore subtùs, remigum pogoniis internis, rectricumque basi rufis; rostro albo, culmine apiceque fuscis.*
Longitudo 5.

Genus *Emberiza.*

68. *Emberiza Baghaira.* *Baag-geyra Lark*, Lath.

This bird is the common *Ortolan* of India, called *Baghairi.*

69. *Emberiza Gingica*, Gmel. *Duree Finch*, Lath.

70. *Emberiza cristata*, Gould's Century of Himalayan Birds.

71. *Emberiza Bengalensis.* *Baya Berbera*, Asiat. Res. *Loxia Bengalensis*, Linn.

The Hindu name of this bird is *Baya*; its Sanscrit name *Berbera.*

Genus *Fringilla.*

72. *Fringilla Amandava*, Linn. *Le Bengali Piqueté*, Buff.

73. *Fringilla formosa*, Lath. *Lovely Finch*, Lath.

74. *Fringilla Malabaria*, ——. *Loxia Malabaria*, Linn. *Malabar Grosbeak*, Lath.

75. FRINGILLA FLAVICOLLIS. *Fring. suprà cinereo-grisea, subtùs albida; jugulo maculâ flavâ notato; humeris ferrugineis; alis maculis albis fascias duas exhibentibus notatis.*
Longitudo $5\frac{3}{10}$.

This bird, though placed amongst the *Finches*, differs in the form of its bill, and it may perhaps hereafter be found expedient to remove it.

Genus *Ploceus.*

76. *Ploceus Philippinus*, Cuv. *Philippine Grosbeak*, Lath.

Fam. *Sturnidæ.*—Genus *Pastor.*

77. *Pastor roseus*, Temm. *Rose-coloured Thrush*, Lath. *Le Roselin*, Le Vaill.

78. *Pastor tristis*, Temm. *Merle des Philippines*, Buff.

79. *Pastor griseus*, Horsf. *Le Martin gris de fer*, Le Vaill.

80. *Pastor Contra* vel *Capensis*, Temm. *Etourneau Pie*, Buff.

81. *Pastor Pagodarum*, Temm. *Le Martin Brâme*, Le Vaill.

Fam. *Corvidæ.*—Genus *Corvus.*

82. *Corvus Corone*, Linn. *Carrion Crow*, Lath.

This bird appears to be the common *Carrion Crow* of India; it differs only in size from the European *Crow*, and in the greater elevation of the bill.

Genus *Coracias.*

83. *Coracias Bengalensis*, Linn. *Blue Jay from the East Indies*, Edw

Genus *Pica.*

84. *Pica vagabunda*, Wagler. *Rufous Magpie*, Hardw.

Fam. *Buceridæ.*—Genus *Buceros.*

85. *Buceros Gingianus*, Lath. *Indian Hornbill*, Lath.

There is some confusion with regard to this bird in Dr. Latham's General History, under the heads of *Gingi* and *Indian Hornbill:* it is the *Dhanesa* of India.

86. *Buceros Malabaricus*, Gmel. *Unicorn Hornbill*, Lath.

There

There is also much confusion with regard to this bird under the heads of *pied Hornbill* and *Unicorn Hornbill* of Latham: it is the *Dhanesa* of the latter, var. B.

Tribus Scansores.

Fam. *Psittacidæ.*—Genus *Palæornis.*

87. *Palæornis torquatus*, Vig. *Psittaca Borbonica torquata*, Briss. *La Perruche à double collier*, Buff.

88. *Palæornis Bengalensis*, Vig. *Psittacus Bengalensis*, Gmel. *Blossom-headed Parakeet*, Lath. sp. 74. var. A.

89. Palæornis flavicollaris. *Pal. viridis; capite lilacino-cano, flavo marginato; rectricibus mediis cæruleis apice albo.*
Longitudo 12.

According to the description, this would appear to be Dr. Latham's *yellow-collared Parrakeet;* but he refers to figures which do not correspond.

Fam. *Picidæ.*—Genus *Bucco.*

90. Bucco caniceps. *Buc. gramineo-viridis; capite, nuchâ, collo, pectoreque griseis; illius plumis in medio albido lineatis; rostro rubro; pedibus flavis; regione circumoculari nudâ flavescenti-rubrâ.*
Longitudo 10.
Fichtel's Barbet, Lath.?

This bird is the *Bura-Bussunta* of India, and appears to be the same as var. A. of Dr. Latham's *Fichtel's Barbet.*

91. *Bucco Philippinensis*, Gmel. *Barbu des Philippines*, Buff.

Genus *Picus.*

92. *Picus Bengalensis*, Linn. *Bengal Woodpecker*, Lath.

93. *Picus Mahrattensis*, Lath., Ind. Orn. *Mahratta Woodpecker*, Lath.

Fam. *Certhiadæ.*—Genus *Sitta.*

94. Sitta castaneoventris. *Sit. supernè griseo-plumbea; pectore abdomineque castaneis; strigâ a rictu per oculos ad nucham extendente, remigibus, rectricumque pogoniis internis nigris; gulâ maculâque rectricum lateralium albis.*
Longitudo 5.
Ferruginous-bellied Nuthatch, Lath.?

Genus *Certhia.*

95. Certhia spilonota. *Certh. suprà griseo-fusca, albo maculata; capite albo graciliter striato; gulâ abdomineque albidis, hoc fusco fasciato; caudâ albo fuscoque fasciatâ.*
Longitudo $5\frac{1}{2}$.

The tail of this bird is soft and flexible, in which respect it differs from the type of the genus, but it agrees in all others.

Genus *Upupa.*

96. *Upupa minor*, Shaw. *La Huppe d'Afrique*, Le Vaill.

Fam. *Cuculidæ.*—Genus *Leptosomus.*

97. *Leptosomus Afer.* *Cuculus Afer*, Gmel. *Edolian Cuckow*, Shaw.

Genus *Cuculus.*

98. *Cuculus canorus*, Linn. *Common Cuckow*, Lath.

This bird, on comparison with the *common Cuckow*, differs so little that

that it can scarcely be called a variety; it is the common *Cuckow* of India, and its habits and note resemble those of the European bird.

99. *Cuculus fugax*, Horsf. *Bychan Cuckow*, Lath.

The common Indian name of this bird is *Pīpīha* or *Pipeeha*, from its note; in Sanscrit *Chataca*. Dr. Buchanan named it *Cuculus radiatus*.

100. *Cuculus Sonneratii*, Ind. Orn.? *Le petit Coucou des Indes*, Sonn.? *Sonnerat's Cuckow*, Lath.?

Not having either specimen or figures to refer to, I conclude, from description alone, that this bird is *Sonnerat's Cuckow*.

Genus *Centropus*.

101. *Centropus Philippensis*, Cuv. *Coucou des Philippines*, Buff. *Chestnut Coucal*, Lath.

This bird is the *Mahooka* of India, so named from its note; it is called also, by the English, *Pheasant Crow*. Dr. Latham's *chestnut Coucal* very accurately describes it, but his figure is bad; having apparently been taken from a drawing of Gen. Hardwicke's, which stated it to be a young bird. Dr. Buchanan named it *Cuculus castaneus*.

Genus *Eudynamys*.

102. *Eudynamys Orientalis*. *Cuculus Orientalis*, Linn. *Eastern black Cuckow*, Lath. *Coucou noir des Indes & Coukeel*, Buff.

This bird is the *Coel* of India, and the *Coukeel* of Buffon.

103. *Eudynamys Sirkee*. *Centropus Sirkee*, Hardw. *Sirkeer Cuckow*, Lath.

Tribus TENUIROSTRES.

Fam. *Meliphagidæ*.—Genus *Chloropsis*.

104. *Chloropsis aurifrons*, Jard. & Selby. *Malabar Chloropsis*, Jard. & Selby.

This bird is the *Hurēwa* of India, and is well described by Dr. Latham as the *Hurruwa Bee-eater*.

Fam. *Cinnyridæ*.—Genus *Cinnyris*.

105. CINNYRIS ORIENTALIS. *Cinn. capite, collo, dorsoque splendidè virescenti-purpureis; abdomine purpureo-atro; alis caudâque atris; fasciculo utrinque sub alis aurantiaco.*
Longitudo 4.
Eastern Creeper, Lath.

ORDO III. RASORES.

Fam. COLUMBIDÆ.

Genus *Vinago*.

106. *Vinago militaris*. *Columba militaris*, Temm. *Columbar Commandeur*, Temm. *Hurrial Pigeon*, Lath.

Genus *Columba*.

107. *Columba tigrina*, Temm. *Colombe à nuque perlée*, Temm.

108. *Columba Cambayensis*, Gmel. *Colombe maillée*, Temm.

109. *Columba risoria*, Linn. *Colombe Blonde*, Temm. *La Tourterelle Blonde*, Le Vaill.

Le Vaillant mentions a larger bird of this species which is common in

in Africa; the same thing occurs also in India, where there are two birds differing only in size.

110. *Columba humilis,* Temm. *Colombe terrestre,* Temm.

Fam. Phasianidæ.

Genus *Pavo.*

111. *Pavo cristatus,* Linn. *Le Paon,* Buff. *Crested Peacock,* Lath.

Genus *Tragopan.*

112. *Tragopan Satyrus,* Cuv. *Meleagris Satyrus,* Linn. *Horned Pheasant,* Lath.

Fam. Tetraonidæ.

Genus *Pterocles.*

113. *Pterocles exustus,* Temm. *Ganga ventre-brulé,* Temm.

Genus *Francolinus.*

114. *Francolinus Ponticerianus,* Temm. *Francolin à rabat,* Temm.

115. *Francolinus vulgaris,* Steph. *Le Francolin,* Buff. *Francolin,* Edw.

Genus *Perdix.*

116. *Perdix picta,* Jard. & Selby. *Painted Partridge,* Iid. *Beautiful Partridge,* Lath.

117. *Perdix Hardwickii,* Gray. *Curria Partridge,* Lath.

118. *Perdix Cambayensis,* Temm. *Perdrix rousse-gorge,* Temm.

Genus *Coturnix.*

119. *Coturnix dactylisonans,* Meyer. *Common Quail,* Lath.

This bird is named *Ghagul;* it corresponds with the European species, and is not very common in India.

120. *Coturnix Coromandelica.* *Perdix Coromandelica,* Lath. *Perdix textilis,* Temm. *Caille nattée,* Temm.

This is the most common *Quail* of India called *Bhuteir.* Dr. Buchanan named it *Perdix olivacea.*

Genus *Hemipodius.*

121. *Hemipodius Dussumier,* Temm. *Turnix Dussumier,* Temm. *Mottled Quail,* Lath.

Fam. Struthionidæ.

Genus *Otis.*

122. *Otis Indica,* Ind. Orn.? *White-chinned Bustard,* Lath.?

This bird has usually been considered as the female of the *Otis aurita,* and has been so figured and described; but it is well known to be a distinct bird. It is the common *Leek* of India, called by the English *Bastard Florican.* I am not quite certain that Dr. Latham's *White-chinned Bustard* is the bird, but his description is so near, that I have not thought it proper to make a new species.

Ordo IV. GRALLATORES.

Fam. Gruidæ.

Genus *Grus.*

123. *Grus Orientalis,* Briss. *Ardea Antigone,* Linn. *Indian Crane,* Lath.

Fam. Ardeidæ.

Genus *Mycteria.*

124. *Mycteria Australis.* *Ciconia Mycteria Australis,* Hardw. *Tetaar Jabiru,* Lath.

Genus *Ardea.*

125. *Ardea purpurea*, Linn. *Le Héron pourprè huppé*, Buff. *Crested Purple Heron*, Lath.

126. *Ardea speciosa*, Horsf. *Darter Heron*, Lath.

This bird is without doubt the *Darter Heron* of Dr. Latham; and the *Ardea speciosa* of Dr. Horsfield is, I think, merely the Javanese type of the same bird.

127. *Ardea Torra*, Buch. *Ardea Egretta*, Lath. Ind. Orn. var. *Ardea alba*, Linn. var. *Great Egret*, Lath. Indian variety *Torra* or *Bughletar*.

This is the Indian *White Egret*, and it differs only in size from the European species, being somewhat smaller. Dr. Buchanan named it *Ard. Torra*, and when without its filiform appendages on the back, *Ard. Putea*; so that these Indian terms appear to correspond with *Ard. Egretta* and *Ard. alba*.

128. *Ardea Caboga*, Penn. *Caboga Heron*, Penn. *Gibraltar Heron*, Lath. var. A.

The term *Caboga* is a corruption of the Indian term *Gao-buga, Cow* or *Cattle Heron*, in allusion to its frequently being seen amongst cattle, like the *Gibraltar Heron*.

Genus *Botaurus.*

129. *Botaurus cinnamoneus.* *Ardea cinnamonea*, Gmel. *Cinnamon Heron*, Lath.

Genus *Nycticorax.*

130. *Nycticorax Europæus.* *Ardea Nycticorax*, Linn. *Night Heron*, Lath.

Genus *Tantalus.*

131. *Tantalus papillosa.* *Ibis papillosa*, Temm. *Bald Ibis*, Lath.

Fam. Scolopacidæ.

Genus *Rhynchæa.*

132. *Rhynchæa Orientalis*, Horsf. *Cape Snipe*, Lath. *Bécassine de Madagascar*, Buff.

Genus *Tringa.*

133. *Tringa ochropus*, Linn. *Green Sandpiper*, Penn.
134. *Tringa Glareola*, Linn. *Wood Sandpiper*, Penn.
135. *Tringa pusilla*, Linn. *Little Sandpiper*, Lath.
136. *Tringa hypoleucos*, Linn. *Common Sandpiper*, Lath.

Fam. Rallidæ.

Genus *Parra.*

137. *Parra phœnicura.* *Gallinula phœnicura*, Lath., Ind. Orn. *Red-tailed Gallinule*, Lath. *Poule-Sultane de la Chine*, Buff.
138. *Parra Sinensis*, Gmel. *Chinese Jacana*, Lath.
139. *Parra Indica*, Lath., Ind. Orn. *Indian Jacana*, Lath.

Genus *Rallus.*

140. *Rallus niger*, Gmel. *Black Rail*, Lath.

Genus *Porphyrio.*

141. *Porphyrio hyacinthinus.* *Fulica Porphyrio*, Linn. *Purple Water-hen*, Edw.

Fam.

Fam. Charadriadæ.

Genus *Vanellus.*

142. *Vanellus Goensis.* *Tringa Goensis,* Lath. *Vanneau armé de Goa,* Buff.

143. *Vanellus ventralis.* *Charadrius ventralis,* Wagl. *Spur-winged Plover,* Hardw.

144. *Vanellus bilobus.* *Charadrius bilobus,* Gmel. *Bilobate Sandpiper,* Lath.

Genus *Cursorius.*

145. *Cursorius Asiaticus,* Gmel. & Lath. *Courvite de la Côte de Coromandel,* Buff.

Genus *Himantopus.*

146. *Himantopus melanopterus.* *Charadrius Himantopus,* Linn. *L'Echasse,* Buff.

Genus *Charadrius.*

147. Charadrius hiaticuloïdes. *Char. suprà griseo-fuscus ; fasciâ frontali alterâque verticali, corpore subtùs, collarique nuchâli albis ; lineâ sub oculis ad aures extendente, fasciâ ad frontem, torqueque pectorali subgracili ad nucham extendente nigris ; rectricibus, duabus mediis exceptis, albis, in medio nigro et griseo-brunneo notatis, fasciam semilunarem exhibentibus.*

This bird differs chiefly from the European species in size, being at least one third smaller, and in the narrowness of the pectoral band.

Ordo V. NATATORES.

Fam. Anatidæ.

Genus *Anser.*

148. *Anser Indicus,* Lath., Ind. Orn. *Barred-headed Goose,* Lath.

149. *Anser melanotos,* Gmel. *Black-backed Goose,* Lath.

150. *Anser Coromandeliana,* Gmel. *Sarcelle de la Côte de Coromandel,* Buff. *Anas Girra,* Hardw. *Girra Teal,* Lath.

Genus *Anas.*

151. *Anas arcuata,* Cuv. *Siley Teal,* Lath.

The name of this bird in India is *Siley* or *Silhei,* from its whistling note ; the English call it *whistling Teal ;* it scarcely differs from the Javanese species as figured by Dr. Horsfield.

152. *Anas Crecca,* Linn. *Common Teal,* Lath.

This bird is the *common Teal* of India, and agrees exactly with the British species.

Fam. Colymbidæ.

Genus *Podiceps.*

153. *Podiceps minor,* Lath., Ind. Orn. *Little Grebe,* Lath.

Fam. Pelecanidæ.

Genus *Carbo.*

154. *Carbo fuscicollis.* *Phalacrocorax fuscicollis,* Shaw. *Brown-necked Shag,* Lath.

Genus *Plotus.*

155. *Plotus melanogaster,* Gmel. *Black-bellied Darter,* Lath.

Genus *Sterna.*

156. *Sterna melanogastra,* Temm. *Hirondelle de mer à ventre noir,* Temm.

 August

August 23, 1831. Joseph Smith, Esq. in the Chair.

Two letters from Mr. J. B. Arnold of Guernsey were read, containing particulars of his experiments in the naturalization of Sea Fishes in a lake chiefly supplied with fresh water. The area of the lake is about five acres; its depth various; and its bottom also various, being muddy, gravelly, and rocky. The water is during nine months of the year drinkable for cattle, but in consequence of a supply which it receives through a tunnel communicating with the sea, is rather salt in summer, at which season the freshes do not come down so plentifully as at other times. The fishes introduced into the lake have been the *grey Mullet, Sole, Turbot, Brill, Plaice, Basse, Smelt,* and *grey Loach.* All of these have thriven well, and are believed to have increased in numbers: the *grey Mullet* especially is known to have bred as freely as in the sea itself. A single *Whiting* having been caught for three successive years, was found to have grown considerably: a *Pilchard* also throve well. All the above-mentioned fishes were placed in the lake, except perhaps the *Brill;* but others, as the *silver Bream,* appear to have introduced themselves. It is even suspected that hybrid fishes have been produced, as several have been caught which were unknown to persons well acquainted with the species usually met with on the coast of Guernsey. . Mr. Arnold adds that Sea Fishes, after having been naturalized in his lake, have been transferred to ponds of spring water, where they have not only lived, but done well; and that such naturalized fishes have been carried to a long distance, being much more tenacious of life than those caught in the sea.

Numerous specimens of *Hylurgus Piniperda,* Latr., presented to the Society by Barlow Hoy, Esq., were exhibited, together with specimens of the shoots of *Pines* perforated by these insects. The mode in which the young branches are destroyed by these perforations has been illustrated by Mr. Lindley in Mr. Curtis's 'British Entomology'. Its effect was regarded by Linnæus as analogous to that of pruning.

The exhibition of the collection of *Fishes* formed at the Mauritius by Mr. Telfair, portions of which had been brought before the Committee at the Meetings in April, was resumed. From among them Mr. Bennett pointed out more particularly the following species hich he believed to have been previously undescribed.

Serranus Delissii. *Serr. maxillis squamosis; lobis pinnæ caudalis elongatis, æqualibus; radio tertio pinnæ dorsalis producto: supernè stramineus, rubro cancellatim rivulatus, infernè lilacino-ruber; pinnis ventralibus aurantiaco-flavis.*

D. $\frac{10}{17}$. A. $\frac{3}{7}$.

Affinis, ut videtur, *Serr. Borbonio,* Cuv. et Val. Corpus altum, altitudo longitudinis (exclusâ pinnâ caudali) dimidium æquans. Pinnæ pectorales ventrales longitudine æquantes. Præoperculi angulus spinâ unicâ magnâ armatus.

Serranus mitis. *Serr. maxillis alepidotis; radio ultimo pinnarum dorsalis analisque elongato: corpore elongato: argenteus, dorso obscurè flavo-brunneo; pinnis flavo tinctis; dorsali nigro tenuiter submarginatâ.*

D. $\frac{10}{11}$. A. $\frac{3}{8}$. Serr.

Serr. filamentoso, Cuv. et Val., longior: corpus, præsertim ad humeros, crassius: oculus major: vertex rugosus (in *Serr. filamentoso* granulosus tantum): dentes antici superiores conici utrinque quatuor debiliores (in *Serr. filamentoso* majores utrinque duo): color pallidior, flavescens.

SERRANUS TELFAIRII. *Serr. maxillis alepidotis; radio ultimo pinnæ dorsalis analisque elongatis: saturatè roseus, dorso latè citrino maculato, posticè albidus; lateribus argenteo vittatis, guttatimque conspersis; pinnâ dorsali anticè citrinâ, basi roseo-, apice niveo-maculatâ.*

D. $\frac{10}{17}$. A. $\frac{3}{8}$.

Affinis, ut videtur, *Serr. zonato*, Cuv. et Val., quem numero radiorum æquat, cujusque formam, etiam pinnarum, æmulat. Differt picturâ, et præsertim lateribus argenteo vittatis guttatisque.

The latter two species form an interesting addition to a section of the genus *Serranus* distinguished by the elongation of the last ray of both the dorsal and the anal fin. Two other species of this section have been described by MM. Cuvier and Valenciennes, to whom they have only very recently become known. Of one of these, *Serr. filamentosus*, as well as of the two new species above described, specimens are contained in the Mauritius collection.

DIACOPE ANGULUS. *Diac. stramineo-flavescens, infrà pallidior; vittis corporis utrinque septem lilacinis, superioribus obliquis, inferioribus longitudinalibus, 4tâ 5tâque anticè connexis angulum acutum postopercularem formantibus; pinnæ dorsalis parte molli supernè tenuiter nigro marginatâ.*

D. $\frac{11}{13}$. A. $\frac{3}{8}$.

Affinis, ut videtur, *Diac. duodecimlineatæ*, Cuv. et Val.: numerus radiorum idem, vittæque haud operculum signant. Dentes maxillæ superioris externi conici, distantes, subæquales, duo anteriores angulares solum majores; maxillæ inferioris minores, tres laterales medii utrinque majores.

DENTEX LYCOGENIS. *Dent. maxillis transversim dentato-cristatis: dentibus conicis anticis sex, maxillæ inferioris lateralibus majoribus: plumbeus, vittis dorsalibus plurimis argenteis, ventralibus distantibus fusco-flavis; maculâ elongatâ argenteo-albâ sub basi posticâ pinnæ dorsalis; pinnis ventralibus, pectoralibus, dorsali analique anticè rubris, caudali flavidâ.*

D. $\frac{10}{10}$. A. $\frac{3}{8}$.

DASCYLLUS UNICOLOR. *Dasc. corpore alto unicolore nigricante.*

D. $\frac{12}{15}$. A. $\frac{2}{14}$.

Forma *Dasc. marginati*, Cuv. et Val.

HELIASES AXILLARIS. *Hel. pallidè cæruleo-fuscus?; axillâ nigrâ; pinnis, præsertim caudali analique, cæruleo-nigrescentibus.*

D. $\frac{14}{14}$. A. $\frac{2}{12}$.

Affinis, ut videtur, *Hel. anali*, Cuv. et Val. Radius secundus pinnæ analis fortior, sequentes longitudine aliquantulum superans. Corpus ovatum.

JULIS CUVIERI. *Julis caudâ subquadratâ: pinnæ dorsalis radio primo longissimo (quam tertius triplo longiore): rufescenti-brunneus*

neus cæruleo punctulatus; capite virescente, vittis tribus latis rufis; pinnis dorsali analique luteis, sanguineo obliquè lineatis, nigro latè marginatis, cæruleoque fimbriatis; hujus fasciâ marginali lineâ cæruleâ longitudinali mediâ alterâque ad basin notatâ.

D. $\frac{9}{12}$. A. $\frac{3}{12}$. P. 12. C. 13.

This new species of *Julis* is one of those fishes, now becoming numerous, which might be confounded with the *Julis Aygula*, (*Coris Aygula*, LaCép.). The latter appears to have hitherto rested solely on the figure and description preserved by Commerson, no specimen of it having been referred to as existing in collections. A specimen of that species has, however, recently been added to the Society's Museum from a collection of fishes formed in India, and agrees well with the figure published by LaCépède, in the truncation or even sublunation of its caudal fin, and in its general form; in its dried state its colour is uniformly dull blackish brown. This specimen was exhibited in illustration of the distinction between *Julis Cuvieri* and *Julis Aygula*, and also to show that the fish figured under the latter name by Dr. Rüppel differed in various particulars, especially in the rounded form of its caudal fin, from the species indicated by LaCépède. To M. Rüppel's fish, it was remarked, the name of *Julis Ruppelii* might properly be applied.

Anguilla Mauritiana. *Ang. maxillâ superiore breviore, obtusâ; rostro complanato; pinnæ dorsalis initio pectoralibus quam anali propiore; lineâ laterali conspicuâ: dorso fusco pallidoque guttatim marmorato, lineolisque nigrescentibus intertextis notato; pinnis fusco nebulosis.*

P. 18?

Mr. Bennett availed himself of the opportunity afforded by the exhibition of the several species of *Pterois* contained in the Mauritius collection, to bring before the Committee a fish which he had formerly regarded as the *Pterois volitans*, under which name it was included in the catalogue of Sumatran fishes appended to the memoir of Sir T. Stamford Raffles. It formed part of the collection presented to the Society by its founder and first President. It was thus characterized:

Pterois Russelii. *Pter. genis spinosim latè lineato-serratis; osse infra-orbitali antico præoperculoque infernè spinosissimis: cirris parvis sex, nasali utrinque duobusque infra-opercularibus: pinnis pectoralibus caudalis basin attingentibus.*

D. $\frac{13}{11}$. A. $\frac{3}{8}$. P. 13.

Kodipungi. *Russel, Coromandel Fishes*, No. 133.

September 13, 1831. W. Yarrell, Esq. in the Chair.

At the request of the Chairman the following notes of a dissection of the *Alligator Tortoise* (*Chelydra serpentina*, Schweig.) were read by Mr. Martin. They were illustrated by preparations of the stomach; of the *ilium* and *colon*; and of the *cloaca*, with the *penis* and urinary bladders: a drawing of the latter was also exhibited; and a drawing of the throat, representing the *œsophagus* and *trachea* in their natural positions.

"The animal was a male, and most probably young: its length from

from the nose to the *anus* being 1 foot 11 inches, and from the *anus* to the end of the tail 6 inches. The length of the *carapace* was 11½ inches, and its breadth, following the curve, 1 foot 1 inch.

"On the *plastron* being removed, and the *scapulæ* (which are united to it by intervening muscles) being turned back, the heart, inclosed in a peritoneal sac, was exposed; the *scapulæ* in their natural position extending over it like an arch: next, and in the same cavity, (for there was no division either by muscle or membrane,) the liver was seen, divided into two distinct portions, and stretching completely across from side to side: below the liver and occupying what may be called the pelvic portion of the cavity, lay the intestines, among which on the right side was seen the *colon* or commencement of the large intestines enfolding the spleen.

"The heart consisted of one ventricle and two auricles, the right of which was the largest. The figure of the auricles was rounded, each in magnitude equalled the ventricle: both auricles contained coagulated blood. The ventricle was in shape acuminate, of a red colour, and firm and fleshy in structure. Its *carneæ columnæ* were strong, distinct, and numerous, but did not separate it into cells or chambers.

"The liver consisted of two lobes. The right lobe was divided into two parts. On its inferior surface was situated the gall-bladder buried in its substance and containing dull green bile: the duct barely half an inch long. The edge of the left lobe of the liver covered the stomach, which passing under it fitted an elongated furrow in the thick part of the lobe, and was closely united to it by the *peritoneum*. The outer curvature of the stomach was placed in contact with the *parietes* of the *carapace*. The texture of the liver was soft and spongy, easily broken down, and pouring out an abundance of dark green fluid, with which it was saturated. The gall duct entered the *duodenum* 6 inches below the *pylorus*. The under surface of the liver on the right side was connected to the *duodenum*, and partially to the lung on the same side, by peritoneal attachments.

"On the liver being removed the course of the intestines was more fully exposed. Beginning with the *œsophagus*, which immediately on proceeding from the *pharynx* becomes firm and muscular (the fibres being longitudinal), we find it dipping down on the right side of the neck, keeping a straight course, passing under the right clavicle, then crossing below the great arch of the neck within the shell, and passing under the right laryngeal branch to the cardiac portion of the stomach; its length being 7 inches. The *cardium* passes over the left laryngeal branch. The length of the stomach is 7½ inches; the circumference of the thickest part 3 inches; gently narrowing to the *pylorus*. Its texture was firm and muscular, especially at the pyloric portion; and between the peritoneal and muscular coats numerous small white points were observed, which on being cut into were found to arise from the presence of minute worms, of three or four lines in length, coiled up under the *peritoneum*.

"The small intestines were strong and thick: their length 3 feet 11 inches. Their internal surface presented longitudinal *rugæ*. At their

their termination in the large intestines there appeared the rudiment of a *cæcum.*

"Encircled by a fold of the *colon* was situated the spleen, of a dark red colour, and soft spongy structure, almost round in shape, and of the size of a small egg: several tortuous veins proceeded from it, and the veins and arteries of the mesentery in general were of the same character.

"The length of the large intestines was 1 foot 7 inches; the muscular coat was particularly distinct; the villous smooth; and several black patches were observed on its surface, which exhibited great vascularity.

"The urinary bladder was double, or rather it might be said that there were two bladders, lying on opposite sides of the *rectum*, and adhering to the sides of the *pelvis*, each communicating by a distinct opening into the commencement of the *cloaca*. Their size and shape was that of a small pear: their texture very thin and fibrous, the fibres being irregularly disposed.

"The *penis*, 2½ inches long, lay concealed entirely within the *cloaca*. It was grooved along its upper surface with the furrow usual in the *Tortoises*, but instead of being free or disengaged, was attached by a close union throughout its whole length on the under side to the *cloaca*. The *glans* was acuminate, and full an inch from the *anus*. From this union of the *penis* to the *cloaca* it is difficult to conceive that it can ever be protruded externally, especially when its distance from the external orifice of the *cloaca* is considered. The duct of the right bladder, in length half an inch, was found to terminate just above the furrow of the *penis*, while that of the left opened an inch on one side of it.

"The *testes* were about the size of a pigeon's egg, elongated, of a bright ochre colour, and situated in the pelvic portion of the abdominal cavity, one on each side of the vertebral column; their structure was soft and somewhat granular. There were no suprarenal capsules. Beneath the *testes* lay the kidneys, large, irregular in figure, glandular in structure, consisting of brain-like reduplications, and dipping between the interstices of the three lowest ribs, (or rudiments of ribs,) on each side of the vertebral column.

"The palate was smooth, with slight transverse *rugæ;* the *pharynx* wide, simply membranous, and capable of great extension; the tongue a smooth cartilaginous point, at the base of which the *larynx* opened by a very small simple *rima*. There was no *epiglottis;* but around the *rima* a slight fold of the membrane was just perceptible. The *larynx* crossing before the *pharynx* dipped down on the left side of the neck, and passing under the left clavicle, divided into two great branches, at about a foot from the *rima:* the right branch passed before the *œsophagus*, and immediately entered the right lobe of the lungs; the left passed under the cardiac portion of the stomach to the left lobe.

"The lungs consisted of two large and equal lobes, distinct, flat, and dark red, extending from the upper edge of the *carapace* as far as the *pelvis*, but not as in the *Land Tortoises* (the *Indian* and *Greek*, for

for example) attached to the whole inner surface of the shell; their attachment was by one of their edges only to the vertebral column, and slightly to the liver. Their texture was firm, and their cells, though large, were not so irregular as in the *Testudo Græca*.

"Between the lungs passed two singular muscles, retractors of the head, long and slender, which arising one on each side by a tendinous origin from the base of the *cranium* passed on each side of the neck, and coming in contact below its great curve, ran together down the vertebral column, and were inserted into its sides in the spaces between the 6th and 7th and 7th and 8th ribs, each by two distinct fleshy terminations.

"The difference exhibited by this animal in the attachments and conformation of the lungs from the family of *Tortoises* in general indicates an approach, not merely in external configuration, but in internal structure, to the *Alligators*. Nor, although it must be confessed in a degree less striking, is this approach unevidenced by the structure of the urinary organs; the bladder in this species although double is yet small, while its enormous volume in the *Tortoises* in general is a singular feature in their construction: the diminution of volume in this organ seems to afford another indication, not to be overlooked, of an approach to the *Saurian Reptiles*.

"The posterior *nares* opened by two distinct orifices one quarter of an inch from the commencement of the palate and three quarters from the point of the beak: their course was obliquely upwards, and the length of each canal to the external orifice just 1 inch.

"The *os hyoides* consisted of an irregularly shaped body and four arched bones or processes united to it by cartilage; from the anterior part of the body a spinous process partly cartilaginous proceeded to support the rudiment of a tongue. The anterior pair of arched bones were connected to the base of the skull by muscles only; the second pair terminated in a broad and flat extremity, and were more abruptly curved: their use seems especially to support the *pharynx*, and they were not connected to the skull. The first pair were each 4 inches in length; the second little more than 3 inches. The rings of the *larynx* were perfect; the length of the laryngeal branches 3 inches."

LVIII. *Intelligence and Miscellaneous Articles.*

ON MUDARINE.

DR. DUNCAN has published in the Transactions of the Royal Society of Edinburgh for the present year, an account of the active principle of the bark of the root of the *Calotropis Mudarii* or *Mudar*, which he has called *Mudarine*. The bark of this root has been highly esteemed among the natives of India as a specific for the cure of cutaneous and various other diseases; Dr. Duncan has found, however, that it possesses no specific virtue, but that it is nevertheless extremely valuable from its common medicinal properties, which correspond both in kind and in degree with those of ipecacuanha.

To obtain mudarine, the powdered root is to be digested in cold

 rectified

rectified spirit; when the greater part of the spirit has been distilled, the solution becomes deeper coloured, but retains its transparency. As the temperature declines, a white granular resin is deposited by a species of crystallization; the whole is then allowed to dry spontaneously, that all the resin may concrete; the dry residuum is then treated with water, which dissolves the coloured portion, and leaves the resin untouched; the solution contains mudarine.

By exposure to the air, it dries readily, forming a mass of a pale brownish colour, perfectly transparent and homogeneous in appearance, having no tendency to crystallize, but becoming full of cracks, diverging from the centre, exceedingly brittle, and having no adhesion to the capsule containing it, from which it peels off spontaneously. It has no smell, and is intensely bitter, with a very peculiar nauseating taste. It is exceedingly soluble in cold water, at the ordinary temperature of the atmosphere. It is also soluble in alcohol, but the power of this solvent is increased by raising the temperature. It is insoluble in sulphuric æther, oil of turpentine, and olive oil.

It is in the solution in water, when nearly saturated, that the peculiar property of mudarine is most easily exhibited.

At ordinary temperatures this solution is quite fluid and transparent. When heat is gradually applied, it suffers at 74° slight diminution of transparency and limpidity; these changes increase with the temperature, so that at 90° its transparency is nearly lost, and it acquires the consistence of a tremulous jelly; but if it be then suffered to cool, it recovers in a day or two its original limpidity and transparency. If, instead of withdrawing the heat when it has risen to 90°, it be further increased, other changes occur; at 95° it is fully gelatinized, and now there appears to be a separation taking place into two parts, a soft brownish coagulum and a liquid nearly colourless, not unlike the separation of the serum from the crassamentum of the blood, as it spontaneously contracts. At 98° the coagulum contracts in size, while the fluid increases in proportion; at 130° it seems to dissolve; probably, however, it is only reduced in size by contraction; at 185° the coagulum is very small, and has a tenacious pitchy consistency, and at 212° little further change occurs.

The alterations which in this state it undergoes on cooling are next to be observed. At 140° the fluid is very turbid, the coagulum is not diminished, and is hard and brittle; at 110° the fluid is less turbid, the coagulum remarkably brittle, with a resinous fracture; at 100° the fluid is more transparent, with thin detached pellicles on the surface. When cooled down even to the freezing temperature, the coagulum remains unaltered, and very much resembles colophony; but, after the lapse of several days, it gradually liquefies in the portion of fluid in contact with it, without passing through the intermediate form of a jelly. The coagulum, when separated from the fluid, is a transparent brown mass, exceedingly brittle, not deliquescent; fragments angular, lustre resinous, taste bitter and nauseous, adhering to the teeth.

In this state it seems at first not to be soluble in distilled water, but after some days it is dissolved in it, with the same phænomena as in

in the fluid from which it was separated by boiling, and the solution has acquired its original properties. The dry mudarine is readily soluble in rectified spirit, and is not precipitated from the alcoholic solution by the addition of water. As long as any considerable portion of spirit remains, it is not coagulated by increase of temperature; but on allowing the spirit to evaporate by exposure to the air, it remains dissolved in water, and has re-acquired its original properties.

It would therefore seem that its tardy solubility, after being contracted, is owing to the state of increased aggregation; for when this is removed by alcohol, its solubility is quickly restored. Mudarine is also extracted by the action of cold water from the powder, but it is not so easily separated from a gummy matter also dissolved, as from the resin extracted along with it by rectified spirit. Its presence is, however, sufficiently demonstrated by the cold infusion gradually losing its transparency as its temperature is increased, and in this case it regains its former transparency, even after having been subjected for some time to the boiling temperature.

We therefore see, that, in this instance a very active principle is more readily dissolved by cold than by boiling water; and it is probable that there are other instances in which heat is improperly employed, with the view of extracting the active principles of vegetable substances.

PREPARATION OF OXICHLORATE OF POTASH. BY M. SERULLAS.

When chlorate of potash is heated in a glass tube or a porcelain crucible, it fuses, boils, and yields oxygen gas. When the fire is properly managed, and after ebullition has taken place for a certain period, the mass thickens, and a moment arrives at which no more oxygen is given out without increasing the heat: if the operation be then stopped, and the salt dissolved and filtered, a great quantity of oxichlorate of potash is obtained in small brilliant crystals; 40 parts of chlorate yielded in this way 17·5 of oxichlorate. It appears from the experiments of M. Serullas, that chlorate of potash requires a temperature higher than that of boiling mercury for its decomposition, and the oxichlorate a temperature considerably greater.

The moment at which chlorate of potash is converted into oxichlorate, is ascertained by occasionally putting a spatula into the salt, and withdrawing a small portion of it. This is to be powdered, and treated with a little muriatic acid; if it gives a yellow colour, then some chlorate still remains unconverted.—*Ann. de Chim. et de Phys.* Mars 1831.

SEPARATION OF ANTIMONY AND TIN.

M. Gay-Lussac employs tin as a precipitant of the antimony, when the mixed metals have been dissolved in muriatic acid, with a small quantity of nitric acid; muriatic acid being in excess, the antimony is deposited as a black powder, when the tin is immersed in the solution. It requires the application of a moderate heat to produce the separation perfectly; the antimony is to be washed, and dried on the water bath. If the two metals are in solution, and their weight is

not known, one portion should be precipitated by zinc to give the whole of both metals, and another portion by tin to give the quantity of antimony.—*Ann. de Chimie*, xliv. 433.

WHITE'S EPHEMERIS.

To Richard Taylor, Esq. F.R.A.S. F.L.S. &c.

My Dear Sir,

I SHALL be very greatly obliged if you will allow me, through the medium of your extensively circulated Magazine, to correct a vexatious blunder which occurs in White's Ephemeris for 1832, in the first calendar page for August. In the column of Moon's *declination* for that month, her *longitude* is, by a strange mistake, inserted. I entreat the especial favour of your allowing me to insert the correct column of declination in your valuable publication, as in the margin.

Also, at p. 42, January column, *for* 1 moon in perigee *read* 1 moon in apogee, *for* 16 moon in apogee *read* 16 moon in perigee; and insert, 29 moon in apogee.

I am,
My dear Sir,
Yours very faithfully,
OLINTHUS GREGORY.

Royal Military Academy,
Nov. 24, 1831.

Day.	August. Moon's Decl.
1	1°S28′
2	6 7
3	10 21
4	14 1
5	17 1
6	19 13
7	20 34
8	20 59
9	20 29
10	19 4
11	16 48
12	13 48
13	10 10
14	6 5
15	1 41
16	2N.53
17	7 23
18	11 39
19	15 25
20	18 25
21	20 22
22	21 1
23	20 14
24	18 2
25	14 37
26	10 18
27	5 26
28	0 25
29	4 S.29
30	9 1
31	12 59

ON VANADIUM. BY MR. JOHNSTONE.

"An 'Old Correspondent' in the August Number of the Philosophical Magazine and Annals, p. 157, mentions my having found vanadium in an ore of lead from Alston Moor, and suggests that it was more probably from the neighbourhood of Keswick.—On this I have only to remark, that I have never found it, and never said it was to be found in ores of lead, except the two varieties described in the former Number of this Journal, p. 166, as formerly occurring in a now unwrought mine at Wanlockhead."—*Edinb. Journ. of Science*, Oct. 1831. p. 223.

LIST OF NEW PATENTS.

To W. Morgan, York Terrace, Regent's Park, esq., for certain improvements in steam-engines.—Dated the 14th of February, 1831.—6 months allowed to enrol specification.

To

To J. Thomson, Spencer-street, Goswell-street road, gentleman, for certain improvements in making or producing printing types.—14th of February.—6 months.

To T. Bailey, Leicester, frame-smith, and C. Bailey, of the same place, frame-smith, for certain improvements in machinery for making lace, commonly called bobbin-net.—15th of February.—6 months.

To W. Payne, New Bond-street, watch and clock maker, for an improved pedometer for the waistcoat pocket, upon a new and very simple construction.—15th of February.—2 months.

To J. Grime, the younger, Bury, Lancashire, copper-plate engraver, for a certain method of dissolving snow and ice on the trams or rail-ways, in order that locomotive steam-engines and carriages and other carriages may pass over rail-roads without any obstruction or impediment from such snow or ice.—21st of February.—6 months.

To R. Burgess, Northwich, Cheshire, M.D., for a drink for the cure, prevention, or relief of gout, gravel, and other diseases, which may be also applied to other purposes.—21st of February.—2 mon.

To S. Dunn, Southampton, engineer, for certain improvements in, or a method of, generating steam.—21st of February.—6 months.

To R. Trevithick, St. Aith, Cornwall, engineer, for an improved steam-engine.—21st of February.—6 months.

To R. Trevithick, St. Aith, Cornwall, engineer, for a method or apparatus for heating apartments.—21st of February.—6 months.

To W. Sneath, Ison Green, Nottinghamshire, lace-maker, for certain improvements in, or additions to, machinery for making, figuring, or ornamenting lace or net, and such other articles to which the said machinery may be applicable.—21st of February.—6 months.

To R. Abbey, Walthamstow, Essex, gentleman, for a new mode of preparing the leaf of a British plant, for the producing a healthy beverage by infusion.—21st of February.—6 months.

To W. Furnivals, Wharton, Cheshire, esq. for certain improvements in evaporating brine.—21st of February.—6 months,

To J. Phillips, Arnold, Nottinghamshire, servant-man, for certain improvements on bridles.—21st of February.—6 months.

To R. Williams, College Wharf, Belvidere Road, Lambeth, Surrey, engineer, for certain improvements in steam-engines.—28th of February.—6 months.

To D. Seldon, borough of Liverpool, merchant, for a certain improvement or improvements in machinery used to give a degree of consistency to, and to wind on to, bobbins, barrels or spools, rovings of cottons, and the like fibrous substances. Communicated by a foreigner.—26th of February.—6 months.

To D. Napier, Warren-street, Fitzroy-square, engineer; and J. Napier and W. Napier, Glasgow, engineers, for certain improvements in machinery for propelling locomotive carriages.—4th of March.—6 months.

METEORO-

LUNAR OCCULTATIONS FOR DECEMBER.

Occultations of Planets and fixed Stars by the Moon, in December 1831. Computed for Greenwich, by THOMAS HENDERSON, *Esq.*; *and circulated by the Astronomical Society.*

1831.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean solar time.	Immersions. Angle from North Pole.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean solar time.	Emersions. Angle from North Pole.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Dec. 8	o Capricor.*	6	2408	21 30	4 23	174	185	21 57	4 51	214	229
10	42 Aquarii	6	2653	2 6	8 51	95	128	Under	horizon.	...	...
15	ξ^2 Ceti.......	5	255	23 58	6 23	103	76	1 6	7 31	307	291
	μ Ceti.......	4	293	8 56	15 20	125	164	Under	horizon.	...	...
16	*f* Tauri	5·6	379	2 50	9 11	53	46	3 28	9 49	349	350
17	γ Tauri.....	3·4	478	21 19	3 37	99	61	22 10	4 27	293	253
	75 Tauri ...	6	508	0 59	7 16	128	90	1 59	8 17	268	238
	θ^1 Tauri	5	510	1 18	7 35	18	343	☽ almost touching Star.—Occulted to places further North.			
	(99) Tauri	5·6	516	1 55	8 12	61	29	2 42	8 59	333	310
	Aldebaran	1	528	4 46	11 2	48	53	5 25	11 42	340	355
18	119 Tauri	5·6	663	0 58	7 11	120	79	1 55	8 9	266	227
	120 Tauri	6	667	1 27	7 40	96	56	2 29	8 42	290	254
20	g Geminor.	6	951	2 49	8 55	35	354	3 23	9 28	325	284
22	Regulus....	1	1209	13 44	19 39	68	105	14 45	20 40	250	290

* Double Star.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1831.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·428. Oct. 17. Wind N.—Min. 29·231. Oct. 1. Wind S.
Range of the mercury 1·197.
Mean barometrical pressure for the month .... 29·904
Spaces described by the rising and falling of the mercury............ 4·865
Greatest variation in 24 hours 0·594.—Number of changes 16.
Therm. Max. 68°. Oct. 6. Wind S.W.—Min. 40°. Oct. 29. Wind N.W.
Range 28°.—Mean temp. of exter. air 57°·58. For 31 days with ☉ in ♎ 59·68
Max. var. in 24 hours 19°·00.—Mean temp. of spring water at 8 A.M. 54·30

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 12th..... 97°
Greatest dryness of the atmosphere, in the afternoon of the 5th...... 61·0
Range of the index .... 36·0
Mean at 2 P.M. 72°·6.—Mean at 8 A.M. 78°1.— Mean at 8 P.M. 82·0
—— of three observations each day at 8, 2, and 8 o'clock .... 77·6
Evaporation for the month 1·60 inch.
Rain in the pluviameter near the ground 4·835 inches.
Prevailing wind, South-west.

Summary of the Weather.

A clear sky, 2; fine, with various modifications of clouds, 13; an overcast sky without rain, 8; foggy, ½; rain, 7½.—Total 31 days.

Clouds.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
19	7	30	1	19	24	23

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1½	½	2	1	7	12	7½	½	31

General Observations.— This month has been remarkably mild for the season, but windy and very wet, it having rained more or less on twenty-three days, and the amount is one inch and a half more than the mean depth of October for a series of years; the rain was often accompanied with brisk gales from the South-west. From the absence of frost the mean temperature of the external air this month is four degrees higher than the mean of October for many years past; and the temperature of spring water has decreased very little from its maximum for the year. A large lunar halo presented itself several hours in the evening of the 16th, and solar halos on the 28th and 30th.

On the 29th at 10 P.M. an aurora borealis appeared, and rose slowly till a quarter past 11, when its lower arch was at its greatest height in the magnetic north, viz. about 16° and 73° in extent on the horizon. At this time a few columns of light ascended from beneath the arch, but they were thin and rather faint, and did not reach much higher than the head of the Dragon. By 12 o'clock the aurora had disappeared, and was followed by a gale of wind in less than twenty-four hours.

The atmospheric and meteoric phænomena that have come within our observations this month, are, one lunar and two solar halos, eight meteors, two rainbows, an aurora borealis, and eleven gales of wind, or days on which they have prevailed, namely, two from the South, seven from the South-west, and two from the West.

REMARKS.

London. — October 1. Cloudy and warm: rain. 2. Cloudy: fine. 3, 4. Fine: rain at nights. 5—7. Fine. 8. Cloudy: heavy rain and thunder at noon: foggy. 9. Overcast: fine. 10. Fine. 11. Rain. 12. Continued rain, becoming very heavy at night; depth amounting to the unusual quantity of one inch in twenty-four hours. 13. Rain: cloudy and windy at night. 14. Fine: showers. 15. Stormy and wet: clear. 16, 17. Fine. 18. Foggy. 19. Very fine. 20. Overcast: fine. 21. Rain: clear. 22. Stormy, with rain. 23. Rain: clear. 24. Fine: overcast. 25. Rain: clear and fine: windy, with rain at night. 26. Slight showers: rain and thick fog at night. 27. Rain. 28. Very fine. 29. Slight fog: fine. 30, 31. Fine.

Penzance.—October 1. Fair: showers. 2. Fair. 3, 4. Rain. 5, 6. Fair. 7. Rain: fair. 8. Showers: fair. 9. Fair: rain. 10. Fair: showers. 11. Fair. 12. Fair: showers. 13. Fair: rain. 14—17. Fair. 18. Rain. 19. Fair. 20. Rain. 21. Fair. 22, 23. Rain. 24. Fair. 25. Showers. 26. Rain. 27. Fair: showers. 28—30. Fair. 31. Fair: showers.

Boston. — October 1. Rain: rain early A.M. 2. Cloudy: rain early A.M. 3. Cloudy: rain at night. 4, 5. Fine: rain early A.M. 6. Rain. 7. Fine. rain early A.M. 8. Fine: rain P.M. 9. Cloudy. 10. Fine. 11. Cloudy. 12. Cloudy: rain P.M. 13. Cloudy. 14. Rain and stormy. 15—17. Fine. 18. Foggy. 19, 20. Cloudy. 21. Rain. 22. Fine: stormy night. 23. Cloudy: rain A.M. and P.M. 24. Fine. 25. Cloudy. 26. Fine. 27. Cloudy: rain at night. 28. Fine. 29. Cloudy. 30. Fine. 31. Cloudy.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1831.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Oct. 1	29·295	29·246	29·10	29·10	29·324	29·231	28·65	72	55	63	55	65	58	62·5	s.	se.	se.	sw.	...	0·03	0·140	0·020	0·20
2	29·527	29·331	29·30	29·10	29·516	29·346	28·72	70	47	63	54	67	55	62	s.	s.	s.	w.	...	·01	...	·060	·07
3	29·763	29·707	29·60	29·55	29·766	29·709	29·04	70	54	64	54	66	56	57	sw.	sw.	sw.	w.	0·30	·30	...	·380	...
4	29·895	29·809	29·78	29·78	29·920	29·879	29·11	67	52	62	52	62	54	58	s.	w.	sw.	nw.	...	·08	1·145	·010	·41
● 5	30·038	29·907	29·84	29·84	30·058	29·953	29·24	68	50	62	52	63	55	56·5	sw.	w.	w.	nw.	...	...	...	·015	·10
6	29·928	29·914	29·80	29·76	29·947	29·923	29·29	68	58	65	55	68	60	57	s.	sw.	sw.	nw.	·15	·06	...	·100	·05
7	29·718	29·633	29·50	29·50	29·733	29·711	29·11	73	51	62	58	67	58	62	s.	sw.	s.	s.	...	...	...	·040	·07
8	29·708	29·633	29·60	29·55	29·695	29·684	29·13	58	45	62	54	61	53	59	e.	w.	w.	calm	...	·63	0·180	·510	...
9	29·702	29·666	29·60	29·55	29·763	29·656	29·06	65	50	59	48	61	54	55	s.	s.	sw.	calm	·15	·10	·710	·220	·38
10	29·662	29·545	29·55	29·55	29·674	29·544	29·00	62	56	62	52	62	57	55·5	s.	sw.	sw.	nw.	...	...	·105	·040	...
11	29·734	29·710	29·60	29·60	29·743	29·729	29·05	62	54	61	54	61	57	56	s.	sw.	s.	nw.	...	·22	...	·570	...
12	29·734	29·682	29·60	29·50	29·740	29·668	29·13	66	49	60	53	64	55	57	s.	se.	sw.	w.	·10	1·00	·135	·880	...
☽ 13	29·684	29·575	29·44	29·30	29·668	29·591	29·07	68	61	59	54	65	60	56	s.	s.	s.	calm	...	·06	·325	·210	·22
14	29·785	29·620	29·50	29·40	29·705	29·634	28·91	65	55	62	54	64	55	59·5	s.	w.	sw.	w.	...	·05	...	·100	·20
15	30·000	29·594	30·00	29·75	29·978	29·793	29·17	65	41	61	53	62	48	57	s.	nw.	sw.	w.	·15	·02	...	...	·02
16	30·314	30·128	30·28	30·10	30·342	30·206	29·52	67	43	59	50	61	48	54	s.	nw.	nw.	w.	...	...	...	...	...
17	30·423	30·371	30·30	30·30	30·428	30·368	29·68	65	49	60	49	62	54	54·5	sw.	e.	w.	w.	...	...	...	...	...
18	30·418	30·395	30·20	30·15	30·392	30·351	29·80	64	53	62	54	62	55	55	s.	se.	e.	calm	·15	...	·090	...	...
19	30·264	30·074	30·10	30·10	30·228	30·083	29·66	61	45	62	52	62	56	60	se.	se.	e.	calm	...	...	...	...	...
20	30·002	29·941	29·90	29·80	30·033	29·929	29·35	63	53	59	52	62	54	53	s.	sw.	s.	calm	...	·10	·140	·210	...
○ 21	30·035	29·937	29·85	29·85	30·015	29·967	29·25	60	37	55	48	60	49	56	sw.	w.	w.	w.	·15	...	...	...	...
22	30·063	29·916	29·90	29·85	30·083	29·967	29·42	61	51	60	48	59	56	48	s.	sw.	sw.	w.	...	·15	·270	·150	·06
23	29·949	29·825	29·90	29·85	29·995	29·925	29·26	62	46	61	55	62	50	56	w.	sw.	sw.	sw.	...	·11	·320	·230	...
24	30·099	30·057	30·00	30·00	30·136	30·115	29·45	61	53	58	48	58	55	47	w.	nw.	w.	w.	·15	...	...	...	·28
25	29·899	29·595	29·80	29·70	29·883	29·639	29·24	62	51	58	50	60	53	54	s.	sw.	s.	w.	...	·17	...	·370	...
26	29·641	29·535	29·40	29·40	29·646	29·534	28·91	61	50	55	46	60	53	52·5	s.	sw.	sw.	w.	...	·52	·790	·320	...
27	29·739	29·692	29·50	29·50	29·704	29·696	29·15	61	46	55	47	57	50	52	s.	sw.	s.	nw.	·15	·16	·265	·340	...
☾ 28	30·240	30·099	30·15	29·90	30·298	30·120	29·41	62	39	56	47	58	47	47·5	sw.	w.	w.	nw.	...	·04	...	·010	·40
29	30·315	30·288	30·22	30·20	30·330	30·288	29·75	61	31	56	47	59	40	48	sw.	nw.	w.	calm	...	...	...	·040	...
30	30·309	30·220	30·22	30·15	30·318	30·275	29·73	60	41	55	45	54	50	42	sw.	w.	w.	w.	...	...	...	...	...
31	30·211	30·187	30·15	30·10	30·255	30·215	29·64	60	48	55	47	59	52	49	sw.	w.	sw.	calm	·15	...	·060	·010	...
	30·423	29·246	30·30	29·10	30·428	29·231	29·25	73	31	65	45	68	40	54·7					1·60	3·81	4·675	4·835	2·46